Foundations of theory-formation in criminology

methods and models
in the social sciences

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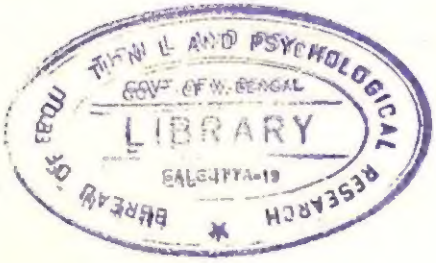
foundations of theory-formation in criminology

a methodological analysis

by C. I. DESSAUR

University of Nijmegen



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Get at your work, all of you, cried the prefect of studies from the door. Father Dolan will be in every day to see if any boy, any lazy idle little loafer wants flogging. Every day.
Every day.

(James Joyce, *A Portrait of the Artist as a Young Man*, New York 1916)

Foreword

The interesting thing about this book for criminologists is that instead of summing up the results of empirical studies or literary research in particular fields of criminology, the author reflects on the course taken by criminology during the past thirty years. Although it is a completely different type of book, I consider it to be an analytical addition to Radzinowicz' rather descriptive *In Search of Criminology* (1961).

An apt question in this foreword would be to ask whether any other branch of science has undergone such drastic changes as ours in such a short period of time. The honest answer would have to be 'yes'. But certainly there is a huge difference between criminology now and formerly; and even twenty years ago has already become 'formerly'.

Infinitely more is being produced in increasingly wider fields. We have been obliged to watch and accept the considerable spread of the subject matter of criminology; we have been obliged to recognize the victimological aspects within – and not outside of – criminology. We have long ceased to base our theories and work on the concept of *crimen naturalis*; we now assume that it must be primarily political facts and circumstances that enable us to label certain behavior as criminal. Once our laws have been made, we proceed to enforce them with our own chosen slackness of determination.

The state of flux in which we find ourselves is aggravated by the fact that in criminology, as in other branches of science, the new methods of research that are continually being developed are seized upon eagerly by the criminologists, who try them out in practice. Furthermore, all those methods are being applied so extensively that our production has become immense. As a result of all this, new criminological theories are being

developed and introduced and it is reasonable to expect that this process will continue to expand. In view of this fact we shall be well-advised to pause a moment and reconsider a number of essential items.

If anyone reading this book should feel that undue criticism has been focused on any criminologist, let him remember that in science the topic is the subject matter itself and not the individual presenting it.

University of Leiden
June, 1971

WILLEM H. NAGEL

Preface

1. Criminology is one of the very few sciences today that does not mean the same to researchers all over the world. It shares this dubious fame with non-empirical sciences like philosophy and dogmatic theology, and with applied psychopathology, viz. psychiatry.

Even though in many empirical sciences (even in the axiomatic deductive or 'formal' ones like logic and mathematics) there are different schools of thought, and different methodological preferences too, one can hardly say that their subject matter itself is still a topic for discussion, or that certain views concerning the subject matter hamper empirical research and theory-formation. However, this is exactly the trouble in criminology.

2. Criminology was born in Europe, where it went through the usual speculative, pre-scientific phase, and where, in the 19th century, modest empirical approaches started.[1] At present empirical research is done predominantly in the USA, where criminology forms part of the departments of social science. In Europe the trend towards modern empirical research is manifest in some Scandinavian, British and Dutch universities, but in most European universities hard-core, up-to-date empirical research is quite the exception.[2] When an attempt at research is made, it often turns out to be of the very low level descriptive kind, without any testing of hypotheses that have been formulated in advance.

In most European universities the early lead, as is so often the case, has turned into a handicap. At a time when social science departments were non-existent, no harm was done by putting criminology in the law school. Today, now that an extensive methodology has evolved for the behavioral sciences, jurists can no longer cope with the technical knowledge

that is required to do up-to-date empirical research in criminology. For this reason, the European jurists who are by tradition the criminologists *par excellence* engage people who have been trained to do behavioral research outside criminology, add their own technical legal knowledge, and produce what they call 'interdisciplinary research'.

3. In other empirical sciences the term 'interdisciplinary research' is used when scientists from different disciplines study the same subject. The curious thing in criminology is that so-called interdisciplinary activities are mostly far from focused on the same subject.

A physicist, a chemist and a biologist may all study, say, metabolism in animals. But the biologist studying the genes of men adjudicated for murder, the psychologist testing girls arrested for 'indecent behavior', and the sociologist studying the interaction between the penal system and the quantity of (detected) criminal behavior are not working on a common subject. The biologist from our example investigates men who happen to be adjudicated for a type of crime that has objective indicators. The psychologist investigates girls who happen to be adjudicated for a type of crime whose definition is dependent on the subjective discretion of the judges.[3] The sociologist in question does not study individual persons at all, but the interaction between a social institution and deviant behavior defined as criminal in general.

If jurists, who usually supervise criminological research in Europe, want to call these diverse activities 'interdisciplinary' or think they can integrate them within their non-empirical legal context, they are of course free to do so, but given the common usage in science, the term is misleading.

What if, as is sometimes the case, an interdisciplinary team studies the *same* criminological object, say the population of penal institution *X*? Will the biologist, the psychologist, the sociologist, the jurist and whoever else is called in then at least produce tested hypotheses about the same subject matter?

It seems to me that in such a case, which is more rare than one would suppose when reading the advocates of 'interdisciplinary' approaches in criminology,[4] the term 'interdisciplinary' is rightly used, except that instead of 'criminology' one should read 'applied criminology' to be exact.

4. The task of fundamental, ethically neutral empirical science is to look behind the obvious questions. It is a good idea especially in behavioral

science to begin by asking: 'Why is something considered a problem? Why is the problem formulated? Why is it formulated in this way? By whom?'

In fundamental criminology it would be good to realize at the onset that the research problems are often formulated by the judiciary, that is by one of the participants in the interaction process that creates criminality, or by other interested parties like bankers who want to combat check-forgers, or by the research team itself which, as a member of the interested party 'social establishment' feels uneasy about the crime problem.

If the geneticist, the test expert and the sociologist were investigating together the parties who suggest criminological problems for research and the way in which they formulate them, perhaps one could justly speak of an interdisciplinary, fundamental criminological science. However, when we confine ourselves to trying to help the current judicial establishment with their social engineering, we may be doing something very useful, but we are bound nonetheless to the current ideology and ethics, bound to pragmatic considerations; in other words: we are doing applied research.

It is for these reasons that I cannot agree with either G. Th. Kempe or L. Radzinowicz (among many others) when they defend the opinion that criminology is essentially an interdisciplinary science. For historical reasons *applied* criminology has become an interdisciplinary science in Europe. Fundamental criminology is seldom considered outside the USA and Scandinavia.

5. I have tried to indicate in this book how in my opinion criminology as a fundamental science should be approached.

The typical, historically determined European problems in criminological research do not obtain in the USA, but I was also unable to rely heavily on the American literature because in that country a more general sociological problem plays a role in criminological research.

In the USA sociological research at the experimental (or semi-experimental) level is often done excellently with the help of the most advanced statistical techniques. However, the interpretation of the statistical results at a theoretical level is generally lacking and this gap between grand verbal 'theories' on the one hand meticulous statistical research on the other hampers criminology as much as the other branches of sociology.[5]

I have tried to indicate a solution to this problem as well, though, to quote a hackneyed phrase, 'much research still remains to be done'.

NOTES

1. Historical overviews are presented in G. Th. Kempe, *Inleiding tot de Criminologie*, Haarlem 1967 (pp. 1–22), and in H. Mannheim, *Comparative Criminology*, 2 Vols., London 1965.

2. An interesting selection of important empirical research can be found in M. E. Wolfgang, et al., *The Sociology of Crime and Delinquency*, New York 1970², and in N. Johnston, et al., *The Sociology of Punishment and Correction*, New York 1962.

For Scandinavian research in fundamental criminology see e.g. N. Christie (ed.), *Scandinavian Studies in Criminology*, Oslo 1968.

3. As an indication of what I mean by legal subjectivity (a matter which will be treated extensively when explanation and prediction in criminology are discussed), I refer for the moment to the quotation at the front of this book. (*A Portrait* was first published in *New York*, not in Dublin, for fear of the Irish law).

4. In the Netherlands Kempe is one of these advocates (see his *Inleiding*, cited in note 1. According to him criminology is a coordinating, interdisciplinary, autonomous, yet applied science.

In England there is L. Radzinowicz. See for instance his *In Search of Criminology*, London 1961, p. 175: '... criminology is not a primary and self-contained discipline but enters into the provinces of many other sciences which treat human nature and society. Indeed, any advance made in causative research into crime must arise out of advances achieved in these other departments of knowledge.'

The reader should note that no distinction is made in this respect between criminology as a fundamental and as an applied science, and that Radzinowicz talks as a matter of course about 'causative research' without recognizing the causal *model* as such.

Kempe (*Inleiding*, p. 7) cites two Dutch criminologists who are also of the 'interdisciplinary' school of thought: M. P. Vrij and H. Bianchi.

Bianchi is the author of *Position and Subject-Matter of Criminology. Inquiry concerning theoretical criminology*, Amsterdam 1956. According to Bianchi criminology is a study of crime and its perpetrator in a specialized, interdisciplinary way (either theoretically or applied) from the metaphysical viewpoint of *deficiency*.

Only in a superficial sense does Bianchi's book treat the same ground as the present one. I will cite a few lines to illustrate from what a totally different point of view Bianchi writes; this quotation may serve as a sufficient explanation for my not entering into discussion with him.

P. 196/197: 'Crime may thus be seen as a deficient deed directed at an anticipation of the Heavenly Kingdom. ... The relation and the being of crime and sin has not yet been thoroughly depicted by the concept of anticipation. This may follow

from what has been said about sin. Sin is the aversion from God. In the concept of anticipation, however, only the deficient turning to God is described. It is, on the other hand, the aversion from God by which repentance is rendered possible. Repentance is an essential part of man's being in crime and sin.'

The principal philosopher quoted by Bianchi in support of his final theoretical view is M. Heidegger, with J. P. Sartre a close second. Even though Bianchi does not always agree with Heidegger, he discusses him seriously in order to arrive at the 'essence' of crime. From Sartre stem sentences like: 'But whosoever transcends (in) his world-design by the perpetration of crime, does not assume his existence but rather flies from it.'

In my opinion, Heidegger's vague, mystical world-model is useless for exact philosophy, which, being axiomatic and purely deductive, should use well-defined concepts and reasonings that are controllable by logical translations. The same goes for the concepts and reasonings of Sartre. What is already bad axiomatic philosophy cannot be good when used as a theoretical model for a supposed empirical science. At best it is considered good literature by some.

(In case that some readers think by now that Dutch criminology is mainly applied dogmatic theology garnished with some 'meaningless' philosophy, I hasten to add that as early as 1949 W. H. Nagel, who was trained by Vrij, made an important contribution to hard-core empiricism in criminology with *De criminaliteit van Oss*, The Hague 1949; a (demographic) statistical description and analysis. Many empirical criminological studies have been published by him or conducted under his supervision. The same empirical trend has also begun recently at Groningen University under the supervision of the criminologist W. Buikhuisen).

5. For an excellent overview of criminological 'theories' that are, however, for the greater part not yet founded on empirically tested hypotheses, see D. R. Cressey and D. A. Ward, *Delinquency, Crime and Social Process*, New York 1969.

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Introduction

1. In order to explain how criminology can be an exact empirical science and how one can arrive at theory-formation therein, I shall have to make a rather wide digression first. Criminology, at the moment, is still a relatively poorly developed discipline among the younger sciences. Very loose general 'theories',[1] hardly refutable at all[2] on the one hand, and an often low level of abstraction in empirical research on the other (with narrow applicability of conclusions as a consequence),[3] are still its trademarks.

In contrast to L. Radzinowicz who thinks that too much time has been spent defining the subject matter of criminology, especially on 'the Continent', I believe that too little thought has been given to the subject matter of criminology, especially on the Continent.

So as to be able to show how theoretical explanation and prediction might be arrived at in criminology I shall, in Chapter I, first define criminology as I see it:

a) an applied, probably interdisciplinary branch, containing most of the research that has been done under the name of criminology, and;

b) a fundamental branch that should become increasingly important in the future.

The latter is neither an autonomous science (specific subject and/or specific methods are totally lacking) nor an interdisciplinary one. It is primarily the combination of two sociological specialisms: sociology of (penal) law and sociology of deviant behavior. What are at present distinct branches of sociology, like the sociology of social stratification, the sociology of conflict, the sociology of power (political sociology or politicology), the sociology of war (polemology) etcetera, may of course offer additional relevant information.

In Chapter II the role of models in empirical science, and specifically in criminology, is elaborated. Criminology has already made ample use of judicial classification as a 'model', although everyone agrees that this classification is partly based on judicial fictions, is inconsistent, often vague, partly overlapping and inexhaustive.

As a reaction to this various typologies were developed, which were intended to reclassify the judicial elements on a strictly logical basis.[4]

Although inherently there is nothing anti-quantitative in typologies, as H.M. Blalock, Jr. points out,[5] for some historical reasons there is often a gap between the typological sociological 'theory' and quantitative empirical research.

Neutral, preferably mathematical models, should at least be combined with them, in order to advance research and the integration of the 'theoretical'and the (semi-)experimental statistical approaches.[6]

In Chapter III a brief explanation is given of the potentialities of mathematical models in criminological research. Although in general this might seem superfluous, I think in criminology it is still useful to recapitulate what mathematical models (other than those based on inferential statistics) have to offer to description, explanation and prediction. Taking the trouble to get acquainted with them will be rewarded when we want to interpret at a theoretical level the many statistical findings already at our disposal.

In Chapter IV some general remarks about explanation and prediction are made. In Chapter V examples are given of typical explanative arguments in current criminology.

2. I am well aware that redefining the subject matter and criticizing the methodology of criminology, in a closely argued way, lacks the glamour of empirical research. Yet it must be done, I think, before more pragmatic contributions can be made and put into their true perspective.

NOTES

1. Like E. H. Sutherland's 'theory' of differential association or R. K. Merton's anomie 'theory', or the containment 'theory' advanced by W. C. Reckless.

2. 'Refutability' is a concept from modern methodology, as propounded by K. R. Popper (see e.g. his *Conjectures and Refutations*, New York 1968, pp. 228–236), which concerns the way hypotheses and theories should be formulated in order to yield meaningful information. See also R.S. Rudner, *Philosophy of Social Science*, Englewood Cliffs, N.J. 1966, especially chapter II.

3. As an example typical of current criminological research one might consider studies made on personality variables of a relatively small criminal population to inform the judiciary or other interested parties about that population.

4. One of the most important contributions in this respect has been made by Th. Sellin and M.E. Wolfgang, *The Measurement of Delinquency*, New York 1964, which is both typological and quantitative.

5. H.M. Blalock, Jr., *Theory Construction*, Englewood Cliffs N.J., 1969, pp. 30–35.

6. A variety of concepts may correspond with the term 'model' in the current literature. One should make a clear distinction between analogous (or empirical) models on the one hand, theoretical models on the other.
The former, although they may have some heuristic value for a while, are rather dangerous in the long run (e.g. the hydraulic model in elementary electricity, the hydraulic model sometimes used by Freud and the Freudians to illustrate the structure of personality, the organismic model in sociology etcetera).
Theoretical models are heuristic devices too, but their psychological dangers are less. They may be defined as either: *an abstract mapping of data* (data being observed phenomena), or, more precisely: *alternative interpretations of the same calculus of which the theory itself is an interpretation.* (A calculus being an uninterpreted axiomatic system).
The first definition given tells us what models mean for our data, the second what their logical status is.
Models in general can neither be proved nor refuted. They are created or adopted for their heuristic value in generating hypotheses and theories that can explain and predict (relationships between) data. The only technical requirement is that they must be internally consistent.

CHAPTER I

The twofold subject-matter of criminology as a fundamental science[1]

1. In relatively flourishing branches of science such as physics or economics it is customary to distinguish sharply between fundamental (i.e. pure theoretical-empirical) science and its application to factual, circumscribed problems. There is physics, and there are branches of technology; there is economics, and there are its applications such as management and marketing. In the young, relatively non-flourishing behavioral science that criminology is, an insufficiently sharp distinction is often made between fundamental and applied science. It is too often taken for granted that anyone who calls himself a criminologist has an opinion about the humanization of imprisonment, about the improvement of rehabilitation, about the guidance of young people who manifest deviant social behavior. On all these subjects the criminologist is expected to give a (well-founded) value judgment or to help effectively in the realization of current ideologies, i.e. collective value judgments (well-founded or not).

However, it is detrimental to the development of criminology as a science that fundamental scientific practice, free from ethical values, is either neglected or freely mixed with applied science (which by its very nature works with value judgments).

A physicist's standing as a scientist does not depend on his capabilities in the field of technological applications; nor does an economist's scientific standing depend on his talents as a businessman. The criminologist still faces the task of scientific self-emancipation. In the Netherlands at any rate he is still generally regarded, and regards himself, as some species of university trained social worker, as a person who concerns himself with acute social *needs*, with *threats* to the established order, with the *defense* of human values, with the *fight* against disastrous developments, etcetera.

In this context it makes little difference whether he expresses his ethical commitment in ethically normative terms (e.g. 'requital of guilt' or 'rehabilitation of the criminal') or uses tables and graphs to conceal his ethical concern with acute social problems.

The subjects with which a criminologist deals: infringements of judicially institutionalized norms in terms of property, sexuality, personal freedom and integrity, will at all times hold an emotional appeal for human beings and persuade them to pronounce ethical value judgments. Criminology as a pure science, however, must of necessity remain aloof from ethical commitment to, and absorption in, acute social problems in order to focus instead on basic, non-problem-oriented, research.

The above may seem trivial and superfluous until one becomes fully aware of the type of subject commonly dealt with in criminological books and publications, the type of subject broached at criminological congresses, and the type of research assignment that criminologists receive from governmental authorities as a matter of course.

The pernicious confusion of pure, fundamental criminology, with applied criminology (i.e. of basic, non problem oriented analysis with ethically committed factual application) has methodological consequences. Criminologists seem to feel that they are pursued by acute social problems: the 'rising tide' of (juvenile) delinquency,[2] the increasing disturbances of the peace ('public order' in Dutch legal terminology), the increasing seriousness of offenses of a certain type, e.g. those of violence.

This feeling, whether or not substantiated by statistical facts, implies that criminologists feel that they cannot yet cope with the community's needs for explanations, predictions, preventive measures and social therapy. The prevailing feeling is one of insufficient time, manpower and means, although the more important problem is of course the lack of an adequate theoretical basis.

Experts in a wide variety of disciplines – psychiatry, psychology, education, law, social work, sociology and physical anthropology – are consulted and requested to formulate (preferably immediately) solutions to social problems which are considered to be urgent.

Almost from birth the 'interdisciplinary' science has been expected to give immediate practical *help* to the delinquent as well as to his victim and to the community on the basis of course of explicit or tacit value judgments and ideologies.

Since no sufficiently sharp distinction is made between this applied crim-

inology that lacks freedom from ethical values and the criminology that is a fundamental science, there is the growing belief (a fashionable bias which it is difficult to eradicate) that the value-free *fundamental* science of criminology must also be an 'interdisciplinary' science.

Statistical studies of delinquent populations (the predominance of inferential statistics as a mathematical model is another trend in fashion) are usually made from various viewpoints with the aid of multidisciplinary models which range from the genetic and physiological to those from the sociology of deviant behavior and psychoanalysis.

This hotchpotch of models, combined with statistical analysis, is mistaken for fundamental scientific practice. The implied expectation is that multidisciplinary statistical analysis of delinquent populations will make it possible to explain and predict delinquency.

That this approach is the opposite of fundamental science and that it is by no means value-free in spite of the seeming objectivity of statistical analyses is my second thesis (my first having been that applied criminology does not know its proper place).

The fundamental methodological problems of a multidisciplinary approach to pure science will be left undiscussed for the moment. Before reverting to methodological questions, let us first consider how the subject matter of criminology is determined.

In the idiom of set-theory – the most general foundation of modern mathematics – we can speak of the totality of all occurring and conceivable types of social behavior as the universe of social behavior (U). Within this universe we can distinguish sets of social acts, some of which have no elements in common (they are disjunctive) while others show overlappings or coincidences. Of course we choose a methodologically opportunistic point of view in defining what we shall regard as sets of social acts within the universe of social behavior.

Within U we intend to distinguish the set: deviant social behavior (D), i.e. behavior which deviates from conventional social norms within the community studied. Thus we define D as the set of social behavior which is deviant in relation to *some* accepted social norm within the community.[3]

As a sub-set of D we then encounter forms of behavior that are deviant in relation to a judicial norm.[4] Let us call this sub-set J. We then find that J can be regarded as the conjunction of at least two (partly-overlapping) sub-sets, namely: judicially deviant behavior that deviates from the norms of civil law and judicially deviant behavior that deviates from the norms of

criminal law.[5] The above can be illustrated with the aid of a Venn-diagram:[6] (A Venn-diagram is a topological diagram; the metrical proportions are irrelevant).

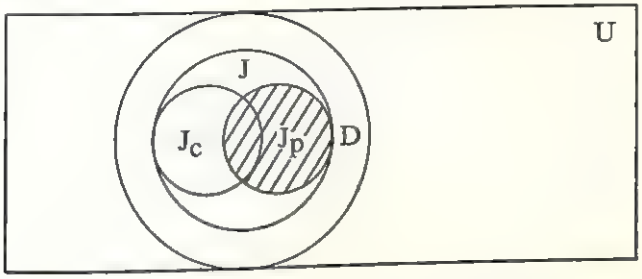

Fig. 4

The spaces in J which are not occupied by J_c (behavior which deviates from the norms of civil law) or J_p (behavior which deviates from the norms of penal law) can be taken to depict behavior which deviates from other judicial norms (e.g. those of constitutional law).

Thus we see that the focal object of criminology is the investigation of deviant behavior which falls within the sub-set J_p.

The criminologist is also interested in horizontal and vertical comparison (terminology of W.H. Nagel): behavior which deviates from norms prevalent in other times and/or other political communities. And the criminologist is also interested in deviant behavior not held punishable in general, insofar as he considers this informative for his studies. His focal object, however, is J_p.

(The efforts to detach studies from prevailing judicial norms in order to focus on norms in general have proved disastrous to the development of the sociology of law, from Petrazhitsky to Weber. These efforts have been in vain).

2. It is an empirical fact that there have always been groups of people some of whose behavior falls within the area marked J_p. The first question which the criminologist must raise is therefore: how is the sub-set J_p within J selected? And its logical corollary: how is the sub-set J within D selected?

This approach to the subject matter of criminology is in the same school of thought as that of, among others, D.R. Cressey and D.A. Ward. In the *Preface* to *Delinquency, Crime and Social Process*, New York 1969, they remark: 'The view of the editors is that *criminality is not "in" people; rather*

it is a status conferred upon certain persons for certain acts at certain times in certain places.'

Also H. S. Becker has (in the words of Cressey and Ward, *op. cit.*, *Introduction* to Part VI: *Societal Reactions to Deviant Behavior*) '... popularized the notion that deviance (which includes more than delinquency and crime) is created in part by the social groups making and applying the rules whose infraction constitutes deviance.'

Once the criminologist has the answers as to who makes the selections involved, who applies the selected rules (the application or in our case: enforcement, is again a highly selective procedure. See Cressey and Ward, *op. cit.*, Part III: *The Administration of Justice as a Selective Process*)and in which way, he can formulate hypotheses regarding the possibility that J_p could prove in fact to be an *empty* set (the utopic community). He can also consider how fluctuations occur in the quantity of forms of behavior that fall within J_p.

If a criminologist wishes to concern himself with the application of his science he can raise the question: how can behavior within J_p be made relatively inconsiderable? But the kind of answer given to that question will depend on the answer given to the questions posed in fundamental criminology.

3. Before giving the first indication of the way in which I would formulate hypotheses as mentioned above, I shall revert to methodology.

We know that criminology as it is now functioning in actual practice:

a) is a relatively non-flourishing science (poor accumulation of research results; poor degree of abstraction; poor practical relevance);

b) usually takes what is called an 'interdisciplinary' approach, or at least regards such an approach as ideal even insofar as it desires to be a fundamental (as opposed to applied) science, free from ethical values.

What we are in fact witnessing is that such deviant behavior as falls within J_p in a given political community, is investigated as profoundly as possible, e.g. by statistical factor-analysis. But we also see that studies as a rule fail to establish how and why J_p comes into being in the first place.

If criminology wishes to be regarded as a science concerned with investigating, describing, explaining and predicting deviant behavior held punishable *hic et nunc* then it should also *investigate* the principal existential condition for that behavior: the fact of its being held punishable.

In a community without penal sanctions there is no 'delinquency' in the

technical sense of the word (even if in such a community there is behavior that can be regarded as deviant in the wider sense).

In a community in which criminal legislation and/or penal policy (detection, prosecution and trial) are intensified, delinquency will increase.

In a community in which criminal legislation and/or penal policy become milder, delinquency in the technical sense diminishes. (For the sake of simplicity, our basic supposition here is that the so-called dark number – behavior defined as punishable but not detected in spite of the authorities' intentions – remains a constant percentage in all cases).

Apart from the primary existential condition for delinquency in the technical sense – criminal legislation and penal policy – there are a number of secondary conditions: the attitudes of the population; the material situation (e.g. during a period of marked petrol shortage there will be little road traffic delinquency; many other material conditions are thus conceivable). However, the primary existential condition or, to phrase it somewhat demagogically, the direct *cause* of the phenomenon of delinquency, is criminal law – even though it is *chronologically* preceded by deviant behavior which one decides to bring into the realm of punishable behavior.

The one-sided attention of current criminology to deviant behavior held punishable, and the virtual absence of empirical research on the reasons for holding a type of behavior punishable and the resulting penal policy, have lead to a methodological dead end. The primary existential condition for 'delinquency' cannot be investigated by any other approach than that of the sociology of (penal) law.

It is not the personal motives of legislators, public prosecutors, judges and law enforcement officers that are primarily relevant to criminal legislation and penal policy. What is relevant is the institutionalized way in which members of certain social groups (in the Netherlands the judiciary could until recently even be described as a *caste*[7]) make use of their social privileges and political power.

The of necessity sociological study of this aspect of criminology should be supplemented by *primarily* sociological studies of deviant behavior (held punishable). Should this behavior be studied primarily as a psychological or 'interdisciplinary' subject, the research results of the two approaches to the criminological object will generally prove to be irreconcilable because the work has been done with two totally different models. To quote Wittgenstein: '... it is generally impossible to pass from one system to another simply by expanding.'[8]

The analysis of the subject matter of criminology as partly the subject matter of sociology of (penal) law, partly of the sociology of deviant behavior, prompts a number of conclusions and remarks which I shall briefly indicate:

a) The relatively poor progress of criminology is a consequence of the insufficiently sharp distinction made between criminology as a fundamental and as an applied (not value-free) science.

b) In applied science there is no objection to an interdisciplinary approach which makes use of quite different models, but in fundamental science, where theory-formation is the ultimate aim, this approach is in general impossible.[9] While additional information from other branches of behavioral science can be utilized, it is necessary in principle to select one or several logically consistent models. The results of investigations using models from different disciplines cannot generally be combined.

c) Perhaps as a consequence of the trend to use an interdisciplinary approach even in purely scientific criminology, the important aspect of sociology of (penal) law has so far been neglected or even overlooked in favor of an exclusive preoccupation with behavior held punishable. Especially *empirical* research on 'the other branch' of criminology is virtually non-existent notwithstanding some 'theoretical' considerations in the same vein as mine, some of which have been cited above.

d) Criminology is not simply an applied science. Nor is it an interdisciplinary or an autonomous science (a unique subject and unique methods are completely lacking). Criminology is a combination of two sociological disciplines: sociology of (penal) law and sociology of deviant behavior.

Chiefly in its applied form it benefits from additional information obtained from scientific disciplines other than sociology, namely: political science, economics, history, social and physical geography, cultural and physical anthropology (the latter heading to be taken as including human physiology and genetics), psycho(patho)logy, penology and pedagogics.

Very little is known exactly about the manner in which the selection of J_p as a sub-set of J and that of J as a sub-set of D takes place. This is a consequence of the deplorable condition of sociology of law in general and sociology of penal law in particular.

The development of sociology of law has had to struggle with two handicaps, namely:

a) The attitude of those (mainly jurists) who maintain that exact empiri-

cal research in this field is either superfluous or impossible. This trend stems originally from a neo-Kantian mysticism which holds that law has some mysterious 'validity' which is not accessible to empirical research. Either because of inadequate sociological training or due to a pseudo self-interest, these people do not recognize that even for factors such as norms, which are not subject to sensory perception, it is possible to find empirical indicators with whose aid these norms can be made accessible to exact empirical research. (Actually, no concept with which we classify our research objects is as such empirically perceptible. For example the concept of a 'generation gap' is not *itself* accessible to sensory perception).

Perhaps the translation of norms into empirical indicators is rather laborious, and requires more intermediate phases, but there is no essential difference from other concepts.

b) The usually rather broad, sweeping design of research in sociology of law that has liberated itself from juristic mysticism. This research focuses on norms in general rather than confining itself pragmatically to the legal norms which prevail *hic et nunc*. Exact investigation of the latter is in any case incredibly comprehensive and complicated (although not more complicated than, say, the study of fundamental particles in physics), and would at this time call for quite considerable investment in terms of manpower and material means.

4. My surmises (preliminary working hypotheses) as to how and why the selection of J_p takes place have been described absorbingly by D. H. Chapman.[10] The contents of his book need not and cannot be described here.

Undoubtedly, the evidence which Chapman presents is limited; more profound studies, preferably making use of the exact and unequivocal idiom of modern mathematics, are required. In his work, however, I recognized my own view that selection of J_p is a relative selection; that it differently affects different groups in a society (the effect on the lower socioeconomic strata is disproportionally strong, whereas the deviant behavior that specifically occurs in higher socioeconomic strata is either not at all judged by the legislator or more likely in terms of civil law; and that it is based in part on the social, economic and psychological interests of socially dominant groups).

The same point of view on the origin and effect of penal law is to be found in D.R. Cressey and D.A. Ward's reader *Delinquency, Crime and Social Process*, New York 1969. In their Introduction to Part II *(The*

Definition of Behavior as 'Criminal') they write: 'The process of defining some form of behavior as criminal involves competition between groups of people participating in the legislative process and in the judicial process.' ... 'Many of our modern criminal laws prohibit conduct that is regarded as taboo – that is, considered morally repulsive – by a vast majority of the members of society. Some contemporary criminal laws, however, have come to institutionalize folkways, values which do not have the broad support such as underlies *mores*. Such legal codification of folkways represents the consensus of only a rather small segment of the society.' (pp. 63–64).

A little further on they write: ... 'the most significant characteristic of white-collar crime is the fact that persons disagree about whether this kind of conduct is in fact crime.' (The next step, if the political opposition manages to get some deviant white-collar behavior defined as crime, is to establish an inefficient enforcement machinery. This social mechanism has been discovered by the Norwegian V. Aubert." In the Netherlands we have recently had some clear examples of it, when a great percentage of physicians broke a (new) law concerning the legal limits of their declarations. In by far most of the cases law breaking by these medical men has not been prosecuted, notwithstanding the complaints of the patients about the excessive heights of the bills, and/or the lacking of specifications which was also made illegal).

It may be necessary to state explicitly that this point of view on penal law does not imply the introduction of a disguised 'leftist' ideology. At least in my opinion, any type of political community will prove to show differential selection of J_p. The groups affected disproportionately may be different: the 'elite' may vary, but the differential nature of criminal law remains regardless of the well known judicial fiction of 'the equality of all before the law'. While in a democratic community we may be all equal before the law, the law certainly is not equal for all of us.

My opinion that research in sociology of penal law should be strongly emphasized in criminology, I found corroborated by the following statement of V. Aubert:[12] 'One main obstacle to the development of a fruitful theoretical orientation is to be found in the tendency to treat criminal behavior, on the one hand, and the system of legal sanctions, on the other, as two separate problems.'

5. Not only does fundamental criminology suffer from a bias towards research into the characteristics of criminals and the want of research into the characteristics of the penal system and its enforcement for lack of critical

thinking, a lack of the relevant 'insight'. Already in 1938 Th. Sellin pleaded for an 'interactionist' approach to criminology (considering both the defining of some deviant acts[13] as criminal and the persons whose acts are thus defined.)[14] And then, E. H. Sutherland's *White-Collar Criminality*, dating from 1939, gave an additional impulse to the interactionist approach.

The fact that until the recent publications of V. Aubert hardly any empirical research was done in criminology, other than the traditional description and analysis of criminal samples, may be seen as a proof that the greater part of the criminologists, belonging to the (in Europe: mainly judicial) establishment, have no interests in directing the attention of researchers (and as a consequence the public) in another way than that of the traditional scapegoats of society.

Although the mechanism may be largely subconscious, it is also remarkable that time and again the variables most deeply investigated are neither the biological nor the sociological characteristics of the criminals, but their psychological make-up. (If one's research was mainly concerned with these biological and/or sociological variables, the selectivity of penal law and its enforcement might become too obvious to be further ignored). Now the remarkable thing is, from the point of view of sociology of knowledge, that already in 1950 a paper was published by K. F. Schuessler and D. R. Cressey that made the negative correlation between criminality and personality characteristics highly probable.[15] Before that time, in 1926, 1931 and 1939 research results had been published demonstrating the negative correlation between intellectual variables and criminality.[16]

The stubborn continuation of non-critical description and analysis of the psychological variables of criminals is a highly interesting fact for a general sociologist, but also a highly frustrating one for the development of criminology. The few psychological differences that may be found for some types of crime between criminals and non-criminals are of course a function (in the mathematical meaning of 'dependent variable') of the criminal legislation that happens to be applied. 'The nature of the norms thus legally sanctioned may, for instance, to some extent determine whether the criminals tend to be rebels, psychopaths, or rational profit seekers.' says Aubert in the article already cited. But in general there are no such differences.

For some types of crime it is sometimes possible to find – e.g. by factor-analysis – mental properties which are characteristic of delinquents that have been adjudicated for committing these crimes; let us say a set

of properties indicated by $\{ a_1, a_2, a_3 \ldots a_i \}$. It will then be found that many individuals with this set of properties, which is considered significant, nevertheless never show the behavior which is held punishable in this context. On the other hand individuals with the set of properties $\{ p_1, p_2, p_3 \ldots p_i \}$ do show the behavior in question.

Where the penal law itself is not directed against a certain psychological type of person (say an exhibitionist or an alcoholic), there are not necessarily significant differences between individuals who are and those who are not detected, prosecuted and tried as delinquents.

The still customary search for the deepest causes of behavior held punishable via psychological analysis of the persons who have been adjudicated for it, is therefore meaningless. Such causes do not exist.

The majority of lawyers will tend, on the basis of their judicial fictions, to regard criminal law as a function of deviant behavior. The criminologist, as a specialized sociologist, is well-advised to regard delinquency (the deviant behavior held punishable) primarily as a function of criminal law. In this way he could make an important contribution to the principal task of sociology: the de-ideologization of the community.[17]

NOTES

1. This chapter appeared, in a slightly different version, as leading article in *Abstracts on Criminology and Penology*, Vol. 9, No. 4 1969, Deventer, the Netherlands.

2. The terms 'delinquency' and 'criminality' with their derivates are used throughout this book interchangeably for people of any age group, according to Continental practice.

3. We define norms as standards by which forms of behavior can be classified as positive or negative. The slightly misleading designation 'behavior which deviates from the norm' actually means: 'behavior which according to the norm is classified in the negative category'.

4. Judicial norms are politically institutionalized norms.

5. My starting point is the type of law system which prevails in my political community (the Netherlands). In this system, the norms of civil and those of criminal law partly overlap.

6. An elementary account of set-theory and Venn-diagrams can be found in J. G. Kemeny, et al., *Introduction to Finite Mathematics*, Englewood Cliffs, N.J. 1966. For a different model (introducing psychological variables) of the subject

matter of criminology see: W. H. Nagel, 'On Criminologists', in: M. E. Wolfgang (ed.), *Crime and Culture: Essays in Honor of Thorsten Sellin*, New York 1968, pp. 11–30.

7. 'Caste' is a concept from the sociology of social stratification. Society is everywhere divided into social classes, that is to say the largest groups of people that have intimate social access to each other. Criteria of class are family membership, educational level, location of residence, type of house, church membership, club membership, size of income, prestige of occupation, manners, dress, speech. When mobility of persons between classes is highly restricted, sociologists apply the term *caste*. Castes are customarily based on race, birth, sex, sometimes on religion and inherited wealth.

There are no absolutely closed castes (not even in India), but for persons with the wrong antecedents, the wrong race, the wrong sex, it may be virtually impossible to become a member of a certain caste.

8. Fr. Waismann, *Wittgenstein und der Wiener Kreis*, London 1967.

9. One should not compare so-called interdisciplinary behavioral research with the now customary interdisciplinary research in fundamental physical and natural sciences. The latter have a theoretical model (the atomic model) in common as their basic presupposition. But a common basic model for say Freudianism and 'differential association' is non-existent. It might even be proved to be principally unattainable, because of their contradictory images of man: the tamed animal whose anti-social instincts may break through anytime, versus the *zoön politicon:* the animal that is socially inclined by its very nature.

10. D. H. Chapman, *Sociology and the Stereotype of the Criminal*, London/New York 1968.

11. See, for instance, V. Aubert, 'White-Collar Crime and Social Structure', in: *American Journal of Sociology*, 58 (November 1952), pp. 263–271. Reprinted in Cressey and Ward, *op. cit.*

To define upper-class deviant behavior legally as crime may be a symptom of social change. To make the enforcement of these new laws as slow and inefficient as possible, is on the other hand a symptom of resistance to social change. By creating new penal laws in this category and by not applying (or hardly applying) any sanctions, social peace is maintained. Both the social reformers and the establishment feel satisfied, Aubert writes.

12. V. Aubert, *op. cit.*

13. See for the process of defining acts as criminal also E. M. Schur, *Crimes Without Victims*, Englewood Cliffs, N.J. 1965.

Schur also sees (criminal) deviance as an interactionist social process and focuses on abortion, homosexuality and drug addiction to illustrate this.

Other authors that have tried to draw attention to the defining process (not with great success if we look at the predominant type of empirical research in criminol-

ogy in most European universities) are: H.S. Becker (see his *Outsiders*, New York 1963). E. M. Lemert (see his *Social Pathology*, New York 1951). E. Goffman (see his *Stigma*, Englewood Cliffs, N.J. 1963).

14. Th. Sellin, *Culture Conflict and Crime*, New York 1938.

15. K.F. Schuessler and D.R. Cressey, 'Personality Characteristics of Criminals', in: *American Journal of Sociology*, 55 (1950), pp. 476–484.

16. C. Murchison, *Criminal Intelligence*, 1926. E.H. Sutherland, 'Mental Deficiency and Crime', in K. Young (ed.) *Social Attitudes*, New York 1931, pp. 357–375. S.H. Tulchin, *Intelligence and Crime*, Chicago 1939.

17. Th. Geiger, *Arbeiten zur Soziologie*, Neuwied 1962. Geiger writes: 'Soziologie ist Ideologie-Kritik'.

CHAPTER II

From judical classifications through typologies to theoretical models

1. At the basis of traditional, 'etiological' criminology there is the judicial fiction that people can be dichotomized according to their being either criminal or non-criminal. Originally this dichotomy may have been almost equivalent to a more implicit one, viz. that in 'good' vs. 'bad' persons, or 'decent' vs. 'sinful' ones. This fictional dichotomy may still be current in public opinion and is so in some criminological literature, as we have seen in the Preface, note 4, in the quotation from Bianchi. According to Bianchi the delinquent is the one who *fails*, because of the etymological connection between this term (used for any age group on the Continent, as I explained in Chapter I, note 2) and the Latin verb *delinquere*, which means 'to fail'. But in less explicit reasonings, like the prediction studies of the Glueck's (which will be discussed in Chapter VI) the same kind of, originally ethical, dichotomy may be implicitly present.

However, this dichotomy is tenable neither from a logical nor from an empirical point of view.

Logically one should object that the selection of which (deviant) behavior is to be defined as criminal is relative; changing from state to state and from period to period. Moreover the enforcement of a given penal law is known to be a partly haphazard, partly extremely selective (biased) procedure. For extensive factual information on this point one should consult Cressey and Ward.[1] Although some of their facts apply predominantly to the USA (like so-called 'plea-bargaining' which lowers the maximum penalty for the accused and eases the proof of guilt for the magistrates) we may be convinced that in general the same sociological mechanisms will be present in any western-type state, such as police bias against certain categories of suspects, and class bias of magistrates.

Therefore we may say that those who become known as criminals have only two things in common:

a) They (are supposed to) have committed deviant behavior of a kind that happens to be punishable by penal law in their particular society at that time;

b) They have been caught and convicted because of a long chain of partly random, partly selective decisions on the side of police, prosecutors, judges.

Although the combination of these two things may eventually lead to the presence of a certain psychological *type* of criminals in our jails, prisons and other penal institutions (but not necessarily so for every type of offense or crime) this does not imply that the significant psychological differences we may find are themselves significant in the sense of 'causally explanative'. (See also Chapter V on this topic). Certainly they are no reason to make broad, sweeping generalizations about 'the' criminal.

Empirically it has been demonstrated that there is a tremendous amount of undetected criminality among otherwise perfectly respectable citizens. Not only does nearly every grown-up violate at some time or other the laws of traffic and the tax laws, also it has been demonstrated that very high proportions of (middle-class) youth and adolescents have committed one or more offenses and/or crimes in the realm of theft, fraude, violence, the trespassing of age limits in sexual contacts and such.[2]

Most adults have committed some acts defined as criminal by their society. Being convicted for it will trigger off a chain-reaction (e.g. via prison infection and hardness of resocialization in a 'righteous' society) that may eventually lead to serious or habitual criminal acts that most adults do not commit. Adherents of the 'labelling theory' and of 'secondary deviance' have stressed this point.[3]

The logical and the empirical objections to dogmatic labelling of criminals and the search for empirical labels for criminals (etiological criminology) imply that in traditional criminology (carefully jotting down all sorts of sociographic and psychological variables about criminals without ever looking critically at their *raison-d'-être*), we shall either draw from the hat what we put into it, or draw nothing at all. By this I mean the following: It is the penal law, its scope, its sub-divisions, the selection of deviant behavior that is made by it and the selective enforcement of it, that determines '... whether the criminals tend to be rebels, psychopaths, or rational profit-seekers', in the words of V. Aubert.[4] In a political community like

the USA, where in most of the states most forms of unconventional sexual intercourse are held punishable by law (even between consenting adults in the case of e.g. homosexuality, even in the case of variant coïtus positions between husband and wife) and where lower-class boys are almost driven by the socioeconomic structure and cultural climate to engage in gang delinquency, one will find a psychologically different type of criminal from the one found in e.g. the USSR, where the penal law is used among other purposes, to suppress creative, original people (writers, other artists, and honest intellectuals) and Jews. In both kinds of states one will find the petty thief, the violent alcoholic, but in so far as their penal law is used for very specific purposes of the dominant group, the psychological differences that one might find between the 'victims' of their penal law systems and the rest of their population might be considerable, yet void of any really significant information.

Where the penal law is used to secure the norms of a debatable ideology, the only thing we can hope to detect by empirical research is, what kind of conduct and/or what kind of people the adherents of this ideology should like to eradicate. We are getting information about the (ideological) interests the penal law is serving, not about the 'psychological causes of crime'.

Where the penal law is prohibiting deviant behavior that is nearly unavoidable for certain people (be it illegally parking one's car where no reasonable provisions are made for it and where public transport is clearly insufficient, or be it unconventional forms of sexual behavior, or be it practising one's religion, or be it writing or saying what one knows or feels to be the truth) we shall get research results that really give us information about the social and cultural structure, not about the people convicted.

Where the penal law aims at widespread misbehavior like using violence against weaker people, we shall get research results that really give us information about the highly selective way in which this type of rules is enforced, not about the behavior itself nor about the many who commit it.[5]

2. The basic dichotomy of penal law, dividing people into supposed criminals versus supposed non-criminals, is a naive starting point for scientific analysis. The selection made by the penal law and its enforcement are both highly arbitrary. Moreover, there exist in the 'non-criminal' group an unknown but high number of people who have in fact committed acts which would have been adjudged criminal if detected. But not only the basic dichotomy, also the narrower classification of penal law of the indi-

viduals considered and detected as delinquents, is often devoid of any empirical content.

Several concepts that are at present in use in, for instance, Dutch penal law, like *verminderde toerekeningsvatbaarheid* ('reduced responsibility': a concept that is often linked with the concept of 'compulsive crime') and the all pervading concept of 'guilt' (instead of causal or functional relationship[6]) do not map any conceivable empirical content. When these concepts are used during a criminal trial by judges or those who advise them, like forensic psychiatrists, they are misleadingly used as if they were descriptive concepts (concepts mapping some empirical content), whereas in reality they are used as imperatives, exhortations or optatives.[7] The unempirical basis of this type of concept has been demonstrated and a highly interesting, though not yet quite satisfying attempt to develop a sociological (and of course empirical) theory of motivation has been attempted by D. R. Cressey.[8] (See also Chapter V).

Next to the void concepts of penal law there are those that are either too narrow or too broad to be suitable for empirical research. In Dutch law, for instance, we use the concept '*zedenmisdrijf*' (immoral offense), but it is only applied by lawyers if the immoral offense is of a sexual nature. (Sadistic immorality, for instance, is hardly ever considered in this context. Movies, pictures or books that are considered pornographic may easily be confiscated and their manufacturers, sellers etc. prosecuted. Only rarely are these measures taken against very sadistic movies, pictures or books, and I believe it is the same in many other countries that have experienced Calvinistic influences, as for example the USA.)

To the behavioral scientist it will seem more logical to avoid such a narrow interpretation of an important concept. He would prefer either to apply the concept of 'immoral offense' to all kinds of deviant behavior considered seriously immoral, or to avoid this concept entirely as a basis for classification.

An example from Dutch penal law of a judicial concept that is, on the other hand, much too vague and too broad for empirical research (the types of deviant behavior that it encompasses having hardly anything in common except their being defined as criminal deviant acts) is what is called '*verstoring van de openbare orde*' (disturbance of the peace).

With this concept highly divergent deviant acts are classified as if belonging to one category:

hindering the legal transport of a corpse to the cemetery;

neglecting, in time of war, to report the intention of another person to desert;
using violence openly and together with others;
disturbing public meetings by making noise;
hiding a foster-child under the age of six months for gain.

Notwithstanding the judicial fiction – for whatever historical or legalistic reasons – that all of these criminal acts deviate from the same norm (maintenance of the peace), the empirical researcher needs more specific categories of criminal behavior.

3. Because of the defects mentioned, official statistics are rarely useful to the criminologist who wishes to do empirical research with regard to deviant behavior defined as criminal. (As a source of information about the penal measures taken by the judiciary and thus as hard-core basic material for some branches of empirical sociology of law, they are of course extremely useful).

The relatively high 'dark number' (changing in its proportion to detected crime for the various legal categories, one may suppose), the loose and ambiguous classification of the criminal acts detected, according to practical judicial and not scientific needs, the changing directives and needs of different administrations which make historical and international comparisons impossible, all make this seemingly exact and hard-core part of judicial administration almost worthless to empirical science.[9]

Requests on the part of criminologists to start keeping scientifically useful records parallel to the judicial ones, have not been met by the judiciary up to now, at least not in the Netherlands.

4. Criminologists, like all behavioral scientists, need exact, unambiguous, empirically useful classifications of their basic material. From this need stem the attempts at 'typologies' of criminal behavior that, contrary to the historically grown, twisted and amended judicial classification that hasto be kept in use for the sake of jurisprudence, try to be logically consistent, clear, exhaustive, free from ambiguities and free from fictions.

In 1966 Th. N. Ferdinand published a critical analysis of typologies in his book *Typologies of Delinquency* (New York). He maintained that: 'Ever since Cesare Lombroso published his original analysis of criminality in the nineteenth century, there has been a veritable torrent of typologies reflecting various viewpoints regarding the nature of deviant behavior. ...

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'Instead of the hoped for convergence, we find typologies based upon legal categories, typologies drawn from psychoanalytic theory, typologies based upon sociological theory and physiological factors and typologies derived impressionistically from empirical data. Each of these attempts to capture the essence of crime and delinquency is certainly valid in its own right, but taken together they do not provide a progressively finer analysis of deviant behavior. Instead they present us with a patchwork of typologies that are either incomparable or contradictory.' (p. 41).

Ferdinand then sets forth not to defend or attack any particular typology, as criminologists are wont to do, but to give an account of their logical nature in general.

Typologies are, not exclusively but predominantly, used in the behavioral sciences. Whereas physical sciences primarily look for *relationships* among their variables (the original periodic table of elements being a typological exception), behavioral sciences are often content with the description of a collection of types that catalogue the various ways in which a given complex of characteristics can be arranged. (p. 44).

This remark, although essentially in the same vein as the one a methodologist (Ferdinand is a criminologist) might use, is a bit unusually worded. In order to appreciate exactly Ferdinand's analysis, we better look first at how an authoritative methodologist of our time defines classifications and typologies. Therefore I shall largely follow the discussion by R.S. Rudner, in his *Philosophy of Social Science*, Englewood Cliffs, N.J. 1966.

Whereas Ferdinand points out the divergence of various typologies in behavioral science, *in casu* criminology, Rudner starts to point out the divergent uses the term 'typology' is put to. His approach is not only one step more abstract, but also in an opposite direction from that of Ferdinand. The latter investigates how typologies hinder or help the eventual aim of all science: theory-formation. Rudner starts to explain what theories are and arrives via the logic of concept introduction, models, definitional and analytical schemata at the logical status of (what passes for) typologies and classifications.

We shall follow Rudner's approach and then return to Ferdinand's analysis of typologies in criminology.

5. Let us start with a definition of 'theory':

Def. 1: 'A theory is a systematically related set of statements, including some lawlike generalizations, that is empirically testable'. (Rudner, p. 10).

For criminologists an important consequence of this definition is that in the first place judicial imperatives are no theory. Nor do basic penal concepts like 'guilt', 'intent', 'reduced responsibility' belong to a theoretical system in the sense of an empirical science.

The way in which judicial concepts are defined in the law (either implicitly or defined by jurisprudence) does not lend them the status of a set of definitions belonging to the language of a theory.

They are what Rudner designates as a *classificational schema* 'a non-theoretical system which lays down conditions for the applications of its categorical (classificatory) terms.' (p. 32).

At best they may lead to propositions that are analytically deducible from them, that is: propositions that permit logical, not empirical proof.

To grasp fully what is meant by 'theory' in the strict methodological sense,[9] we may also look at it from a genetic point of view.

Def. 2: A theory is the (logically) primary interpretation of a calculus.

Def. 3: A calculus is a non-interpreted (strictly formal) axiomatic system.

Def. 4: An axiomatic system is a language consisting of at least:

1. primitives (undefined elements)
2. axioms
3. formation rules
4. transformation rules

(Formation rules determine which statements of the language are to be considered 'well-formed'. The axioms are a sub-set of the well-formed statements. The transformation rules make it possible to derive other well-formed statements from the axioms. These are called *theorems*.)

Def. 5: A purely formal, or uninterpreted axiomatic system is a calculus.

Def. 6: A deductive system is a (correct) interpretation of a calculus, according to semantical rules.

(Note that although jurists reason deductively, law – in this case penal law – is not what is called a deductive system. There is no clear designation of primitive elements and axioms, nor are there any well described formation and transformation rules.

Of course the penal law system is neither a calculus, for the same reasons, to which is added the fact that it is – from the very beginning – an interpreted system).

If it was not theory-formation in criminology but methodology of (behavioral) empirical science that was our primary concern, we would be obliged to compare the rather confusing terminology with regard to 'theo-

ry', 'model' and such, that abounds in methodological literature. For our purpose it seems sufficient to keep roughly in accord with Rudner. Whereas in methodological and scientific literature the term 'model' for instance may be used as just a synonym for 'theory', or as a reference term for any theoretical formulation that is *not* a theory, we shall follow Rudner's definition of it:

Def. 7: A model is an alternative interpretation of a calculus for which the theory is the (logically primary) interpretation.

By this definition we designate what are sometimes called 'theoretical models', that is models which are interpretations of some calculus, thereby ordering or (psychologically) clarifying our universe of discourse and serving a heuristic function. They should be well distinguished from so-called *analogous* models. Theoretical models are based on an analogy between the logical structure of the calculus and the structure of the empirical system under investigation. Analogous models, however, are based on an analogy between one empirical system and another, thus leading to say the picturing of society as 'a big organism', or as 'a self-regulating machine'. The dangers of the latter are obvious if one realizes how easy it is to extrapolate the empirical analogy further than is warranted. (Those who know a little elementary physics may remember the dangers of the hydraulic analogy for electricity. Within sociology many scientists realize the dangers of the organismic analogy, even if they overlook it in functionalism.)

If one tries to evaluate what so-called theories and models in behavioral science really are, one sees that apart from a few very rare exceptions (genetics; learning theory) there are no theories or theoretical models available in most branches, and certainly not in criminology, but either analogous models or non-theoretical statements of one kind or another.

Among the latter we shall now have to distinguish definitional schemata, analytical schemata, classificational schemata and typologies.

6. The many and varied non-theoretic formulations that are used by behavioral scientists in general and that can be found in criminological literature, must be divided into two broad classes:

a) those formulations that are made up of statements (i.e. truth-functional propositions) and

b) those formulations that do not contain any statements at all.

These two classes of non-theoretic statements contain respectively what

Rudner calls *analytical conceptual schemata* and *definitional schemata*, or, synonymously, *analytical schemata* and *definitional systems*.

The difference between analytical schemata and empirical theories is that the former contain no empirical assertions, only logical or analytic truths. The difference between analytical schemata and analytical models is that the former draw their vocabulary from the theory of concern, whereas analytical models use an alternative vocabulary, as follows from the definition of a model.

The difference between definitional schemata and empirical theories is that the former contain no statements (sentences which are either true or false) at all.

A large part of juridical, *in casu* penal language is made up of these two types of schemata. In empirical criminology they may be found too, but more important there are two other types of non-theoretic formulations: classificational schemata and typologies.

A classificational schema makes one or more partitions in its universe of discourse. If it is well-constructed it will be a clear, unambiguous, exhaustive and non-overlapping partitioning.

If a condition is given for the applicability of each of the classifying terms that is both necessary and sufficient, we have, says Rudner, the equivalent of a definitional schema.

If none such condition is given, or only for a few of the classificational terms, we have a certain type of analytical schema.

(One should be aware of the fact that although classificational schemata may be considered – if necessary reformulated – as definitional or analytical schemata, the reverse does not hold).

Most non-methodologists would think of classificational schemata as dividing up the empirical universe of discourse. It would be more in accord with current methodological practice, however, to make what Rudner calls 'the linguistic turn' and think of them as systems that give definitions and/or conditions for their applicability.

As for the analytical classificational schemata, they may have great heuristic value, in the same way that models have, because they may reveal e.g. the absence in empirical reality (the 'contingent' absence) of one or more categories that logically should be present. Also they may psychologically induce one to formulating empirical (testable) hypotheses. In this way classificational schemata may end up as part of real theories, although they themselves are not directly empirically testable.

The last important class of non-theoretic formulations to be discussed here: typologies, may be defined as linguistic systems that *order* the elements of their universe of discourse.

From this definition it becomes evident that a lot of so-called typologies in behavioral science are in fact what Rudner would call either vague, informal formulations, or definitional or analytical or classificational schemata. However, a few typologies that do order their universe of discourse or at least attempt to do so, are present in behavioral science and in criminology.

The concept of ordering is hardly explicable in any other language than that of modern algebra. For an extensive discussion of it I refer to Rudner (pp. 36 – 38).

Now many of the typologies that are or have been current in behavioral science, often using so-called polar types like *Gemeinschaft – Gesellschaft, perfect competition – monopoly, introvert – extravert, urban – rural, folkways – mores, open society – closed society, inner directed – other directed*, have proved to be highly stimulating for discussion, but failed to inspire the formation of any empirical theory in the strict sense. As Rudner points out this is not because of the loose definition of the basic terms (which might be remedied), but because of a lack of explicating the ordering relation that should be applied with them. To paraphrase Rudner's example of this serious failure: how much more other directed are robbers than burglars, prosecuters than thieves?

If one society is predominantly intent on suppressing variations of sexual behavior, but lets a lot of sadistic behavior (or representations of it) unprosecuted and unpunished, while another society is hardly interested in the sexual behavior of its citizens, but keeps women in a legally, politically and socially inferior position, which one is the more open, which one the more closed? (One might think of the USA and the Arab countries as examples.)

But even the best of real typologies, do not offer more than an ordinal ranking (serial ordering in Rudner's terminology) of their universe of discourse. A metrical ordering – one that would enable the researcher in behavioral science to use the same wealth of mathematical tools that physical scientists have at their disposal – is hardly ever found (or even thought of) outside econometrics.

For a metrical ordering to be present all the conditions for a serial ordering must be fulfilled plus some extra conditions.[10]

Certainly in criminology what we have at our disposal are mostly classificational schemata (which may be called typologies by their makers), very few typologies that contain statements about a serial ordering of the universe of discourse and absolutely no metrical typologies.

7. At this point it is fruitful to return to Ferdinand's discussion of specific criminological 'typologies'.

One will remember that one of his statements was that physical sciences look primarily for *relationships* among their variables, whereas behavioral sciences are often content with the description of a collection of types that catalogue the various ways in which a given complex of characteristics can be arranged. (*op.cit.* p. 44).

If we reformulate this in methodologically more precise language, we might transform the statement as follows: 'Whereas physical sciences look primarily for metrical relationships among their variables, behavioral sciences often don't get further than the construction of classificational schemata or, at best, serially ordering typologies.'

Ferdinand's argument then runs on as follows: Although a typology need not be drawn directly from observation, its basis is empirical. It therefore offers more information than a mere classification. (Members of a class may have relatively little in common, see the example from Dutch penal law above). Typologies offer some insight into the 'nature' of their elements, whereas membership of a class reveals relatively little about the nature of a thing. As an example Ferdinand cites the male-female classification, which hardly gives us any information about the nature of the members of the respective classes beyond the strictly biological level.

Again it should be remarked that his dividing line is not quite to the point. For instance the true (serially ordering) typology of open societies versus closed societies does not offer us so much information either, because of defects that have been mentioned above.

Then he argues that typologies are different from theories as well, since the latter are much more abstract and therefore general.

> Whereas typologies try to detect and describe certain uniformities in the physical world, theories go into the *logical* relationships that exist between concepts. (p. 45)

Again these remarks are debatable. The outstanding feature of real empirical theories is not their logical structure. Analytical conceptual schemata and theoretical models show this logical structure as well. As for

generality: a mere classification of things that are alive and things that are not (taking as its universe of discourse all of the material universe that is known to us) would be of the highest generality.

What distinguishes empirical theories from other theoretical formulations and non-theoretical formulations is that they are the interpretation of a calculus and that they contain at least one empirically falsifiable statement. This point is missed by Ferdinand.

8. In a following passage Ferdinand slightly modifies a well-known typology of typologies by C.G. Hempel. According to Hempel there are three types of typologies: the *classificatory*, *extreme* and *ideal* ones.

A classificatory typology (Rudner might say: a classificational schema) merely classifies cases in terms of 'natural' categories, without indicating any order within the 'typology', or indicating any essential characteristics. E. Kretschmer's typology of persons (aesthenic, pyknic, athletic, dysplastic) is given by him as an example.

In extreme typologies types are defined in terms of 'polar characteristics'. W.H. Sheldon's typology of persons (endomorphic, mesomorphic, ectomorphic) is regarded by him as an example of this kind of typology.

Now both these first two types of typologies as distinguished by Hempel are called *empirical* typologies by Ferdinand (although the first gives clear qualitative differences between cases and the second maps a quantitative continuum).

'Both typologies describe the phenotypic patterns that a given population displays, and their construction is essentially an inductive operation. For this reason, they can be regarded as different versions of a more general kind of typology, which I shall label an *empirical* typology.' (p. 47).

Hempel's third kind of typology, the ideal typology, is quite distinct from the other two, says Ferdinand. Although these typologies also describe the actual patterns that are apparent in individuals, the types are 'meaningfully integrated complexes that are derived more or less explicitly from a theoretical structure.' (p. 47).

(What is called a theoretical model in Rudner's terminology is probably what Ferdinand has in mind here).

Ferdinand gives some examples from criminological literature of what he terms empirical typologies. Like all empirical typologies they are inspired by rather obvious, easily discernible characteristics, like physiological traits, anatomical traits (Lombroso) or personality traits (e.g. Hewitt and Jenkins).

'... more elusive characteristics, like the individual's ego-identity or his specific cultural loyalties, have served much less frequently in the definition of empirical typologies.' (p. 49).

As an example of what he and Hempel call ideal typologies, Ferdinand mentions the psychoanalytic typologies of Reiss and Sanford among others. Their defect lies in the fact, that they stem from a specific set of 'theoretical' assumptions. They may suggest explanations for patterns of behavior, but they do not describe precisely what exists. (p. 54).

As a third kind of typology he introduces what he calls the *synthetic* typology, by which he means a typology that coordinates the implications of several ideal typologies, predominantly those of a psychological and those of a sociological bent. As an example he mentions K. Lewin's field theory of behavior.

In current methodological language these 'ideal typologies' and 'synthetic typologies' should probably be called analogous models and eclectic analogous models.

In current methodological language these 'ideal typologies' and 'synthetic typologies' should probably be called theoretical (or sometimes analogous) models and eclectic models.

9. The most surprising element in Ferdinand's discussion of typologies (and their use in criminology) is, apart from its lack of methodological refinement, its lack of philosophical sophistication. From his writing it becomes evident that his starting point is a very naïve theory of knowledge, of the knowledge-is-reflection-of-reality type, which is, to say the least, debatable, and cannot be taken implicitly for granted in the way the author does.

Secondly, and still more remarkable, the author does not for a moment mention the fact that an analysis and discussion of typologies in the way he gives them, is in itself a meta-typology.

As a criminologist Ferdinand is of course not obliged to discuss methodological fundamental issues, but it might have been worthwile in passing to mention that the way in which he describes the typologies found in criminological literature is itself a typology of a particular kind.

I shall discuss these two objections briefly.

With regard to the second objection, it seems to me that Ferdinand as a criminologist is of the 'interdisciplinary' school. What is defined by him as *the summum bonum:* a typology of an eclectic type, combining elements

from several behavioral sciences (the 'synthetic typologies'), is in itself the expression of a certain conception of what criminology is or should be. This interdisciplinary approach has been criticized already in Chapter I above. If, for the moment, we refrain from any criticism of it, we should, however, still stipulate that Ferdinand's typology of criminological typologies is in itself an '*ideal* typology' (in his terminology).

To quote the author: 'Its defect lies in the fact that it stems from a specific set of theoretical assumptions.'

However, only when Ferdinand describes his conception of eclectic ('synthetic') typologies can his own meta-typology truly be called a typology of the ideal type. In his description of empirical typologies in criminology (combining the classificatory and extreme typologies as defined by Hempel) his own meta-typology seems to me to be what he himself would call a typology of the empirical (and probably extreme) kind.

If this analysis of Ferdinand's meta-typology is right, I consider it a fatal blow to the methodological value of his typology. If it is wrong, the fact remains that the author omitted any attempt at an explicit evaluation of his own meta-typology.Neither did he discuss the possibility or necessity that his description and analysis of criminological typologies should itself be exempt from typological rules and definitions.

10. Shortly after the publication of Ferdinand's book M.B. Clinard and R. Quinney published *Criminal Behavior Systems: A Typology*, New York 1967. This book is a reader containing recent criminological publications, and a few important earlier ones, by prominent authors. The editors collected from three to six texts for each of the eight classes of the criminological 'typology' they constructed;

Violent personal crime
Occasional property crime
Occupational crime
Political crime
Public order crime
Conventional crime
Organized crime
Professional crime

There is of course nothing against grouping articles of a reader under some major headings. However, Clinard and Quinney profess to do more than

just conveniently arrange their material. In their *Preface* they say: 'We feel that continued progress in criminology will largely depend on study of *types* of crime.'

They then set out in Chapter I ('Types of Crime') to expound their typology, which is based on distinguishing different criminal behavior *systems*.

Typologies are defined by them as 'classifications, which attempt to specify ways in which variables are empirically connected.'

Classifications are 'sets of variables which are linked to form a number of logically possible combinations.'

In criminology, the authors say, a wide variety of behavior has to be considered. Types reduce the manifold phenomena to more systematic observation and they also may assist in the formulation of hypotheses and serve as guidelines for research. What they then call the theoretical value of a typology would be, in the terminology of this book, its heuristic value as a model.

To the authors the crucial task of what they call theory in criminology (it sometimes encompasses both what is generally called theory and what is generally called theoretical model or even analogous model (see this chapter paragraph 5)) is explanation: 'An adequate explanation of criminal behavior should show not only how the explanation applies to all crime, but how it should be modified to explain certain types.' (p. 3).

It is evident from this supposedly neutral methodological definition at the beginning of the book that the authors implicitly adhere to traditional (and untenable) criminological assumptions.

There is no such thing as crime in general, and therefore no explanation of crime in general apart from its being defined as such by some penal law.

The concept 'crime' encompasses an extremely wide and divergent range of human behavior that changes according to time and place and if there is any explanation to be found for it, one will have to move from specific behavior types to more general categories, if they exist.

Also, in the emphasis the authors put on explanation of criminal behavior as the ultimate aim of criminological science, there seems to be the tacit assumption of a kind of causal model (adhered to by the old etiological school in criminology) that has long been dispensed with in well developed sciences. The latter are not interested in alleged 'causes' whatsoever (the whole concept of cause being most of the time an anthropomorphic, personalistic projection), but in functional or other relationships, either of the deterministic or the probabilistic kind (see next chapter).

The attention of the authors is – as is usual among etiologists – almost exclusively focused on the perpetrator of an (detected) act which has been defined as criminal.

The naïve assumption that it might be possible to arrive at some interesting, intellectually enriching, pragmatically useful explanation of crime in general is characteristic of the etiological school. But what would constitute the common attribute of highly different, free or compulsive, more or less objectively or only purely subjectively observable acts?[11]

The etiologists, blind to the one and only factor common to all criminal acts, namely their definition as such (see Chapter I), are like quantum-physicists who forget their own measuring instruments and activities, or like a physicist who energetically stirs a liquid with a thermometer thinking he is measuring the exact temperature of the liquid.

The types that Clinard and Quinney eventually produce, are nothing but a reshuffled traditional legalistic classification. Moreover they are clearly overlapping. The rabbit finally conjured out of the hat is the one that was put there before the performance started.

By thinking etiologically, by considering crime in general as an existing entity, they do nothing but follow blindly the ideology of sociologically uncritical lawyers. The eight-fold typology they produce (a slightly redefined and slightly reshuffled traditional juridical one) may have just a little more empirical relevance than most current legalistic classifications. Nevertheless it seems to me a dangerous device that could block future research more effectively than any legalistic classification, for the very reason that it is semi-empirical.

11. In a recent monograph by H.M. Blalock, Jr., *Theory Construction: From Verbal to Mathematical Formulations*, Englewood Cliffs, N.J. 1969, a few pages (pp. 30 – 35) are devoted to typologies in sociological science. Blalock's attempt to introduce some mathematics into sociology makes him rather suspicious of typologies, he admits, because of the (not necessary but empirically established) fact that wherever typologies are used mathematics tend to be absent.

Blalock writes: 'Usually, although not always, one can find numerous implicit hypotheses buried among the comparisons of the several types, but it is my own observation that typology construction, for some reason, does not lend itself to an explicit focus on propositions and their interrelationships. There is no inherent reason why this must be the case...'

However, Blalock, notwithstanding his mathematical sophistication, is still aiming at the detection of *causal* relationships in sociology (just like his French colleague R. Boudon in his *L'analyse Mathématique des Faits Sociaux*, Paris 1967). I can hardly share his enthusiasm about the compatibility of the typological analysis of J. C. McKinney, *Constructive Typology and Social Theory*, New York 1966, with his own causally oriented thinking.

On the other hand, if one bears in mind that what McKinney calls a 'constructed type' is roughly equivalent to the *ideal type* as introduced by Hempel and Oppenheim (to name the most famous modern methodologists), Blalock's summary of McKinney's analysis of typologies and his subsequent criticism of Merton's anomie typology can be useful to us.

According to McKinney a 'constructed type' is a 'purposive, planned selection, abstraction, combination, and (sometimes) accentuation of a set of criteria with empirical indicators that serves as a basis for comparison of empirical cases.'

According to V. Capecchi (whose work I have not been able to consult directly, since the journal *Ikon* in which it appeared is not available in the Netherlands) an essential characteristic of typologies is that they reduce a 'property space'. By making a cross-classification of variables and selecting for emphasis only some of the possible combinations, one arrives at a typology. (V. Capecchi, 'Typologies in Relation to Mathematical Models', in: *Ikon*, Supplement No. 58, Sept. 1966).

Quoting Capecchi, Blalock then progresses towards a critical analysis of Merton's well-known typology of deviant behavior, which is as follows:

Pattern	Adaptation to culture goals	Adaptation to institutionalized means
1. Conformity	+	+
2. Innovation	+	−
3. Ritualism	−	+
4. Retreatism	−	−
5. Rebellion	±	±

This typology involves a reduction to only two variables: adaptation to culture goals and adaptation to institutionalized means, both of which have been dichotomized.

This dichotomization, which implies of course a gross simplification, leads to the accentuation of polar types, without subtleties in between, but also without the possibility of zero-values.

Not only are some empirically possible combinations excluded in Merton's typology, some mathematical ones have also been dropped. The four missing cells in Merton's typology, as has been remarked first by V. Capecchi, are those involving (±, +), (+, ±), (±, —) and (—, ±).

These mathematically possible combinations may have been dropped by accident (if Merton overlooked them) or they may have been dropped on purpose, but this is not stated or argued by the author.

If the latter is the case, it means that Merton implied some theoretical assumptions about the incompatibility of for example some adaptation to culture goals, but complete rejection of institutionalized means, and so on.

In Merton's if not widely used, at least widely quoted typology of deviant behavior, we see again that some very debatable assumptions are implicitly present. Also, just as in the criminological typology of Clinard and Quinney, a nominally structured set has been constructed (in the case of Merton's typology an essentially twofold one, plus one intermediate case, in the other case an eightfold one), which does not lend itself to more than superficial empirical application. There is no inherent reason why typologies should not be constructed as a continuum, thus allowing more subtle quantitative applications than mere counting or, at best, ranking. Historically, however, this has seldom been the case in behavioral science.

12. From the point of view of the practising criminologist, both judicial classification and current criminological typologies have their disadvantages. Both tend to contain implicit assumptions that are at least debatable and that may divert the attention of the researcher from the heart of the criminological problem.

Although the empirical relevance of typologies may be somewhat greater than that of the legalistic classification which has grown during a long and twisted historical process, they still do not as a rule lend themselves easily to exact quantitative research. A possible exception to this is the approach used by M.E. Wolfgang and Th. Sellin in *The Measurement of Delinquency*, New York 1964.

By giving weight indices to a supposedly exhaustive set of delinquent acts, they arrived at a measurement device for delinquency, more or less independent from judicial qualifications.

Whereas ordinary 'typologies' at their best are wont to present us with a *nominal* scale that is slightly more representative of empirical facts or that expresses more explicitly some basic assumptions about criminal behavior than the ordinary judicial classification, the measurement device of Wolfgang and Sellin is at the ordinal level. (By applying it, it is possible to rank delinquent acts according to their alleged seriousness, but not yet to speak legitimately of some act being 'twice as serious' as some other. One may compare an ordinal scale with the ranking of people in e.g. a military hierarchy. Although the captain is clearly higher on the ranking scale than the sergeant, one cannot say that he is x-times as high. Nor is it right to say that the distance between the rank of captain and of lieutenant is e.g. 'equal' to the distance between lieutenant and sergeant. Maybe this is so in pay, but then the money-scale – which is a ratio scale – is used.[12] The only statement allowed concerning the socially hierarchical scale is that one military position is higher than the other, and that the ranking relation is transitive: if a is higher than b and b is higher than c, then a is also higher than c. The same goes for the 'seriousness' of delinquent acts measured on an ordinal scale).

The advantage of an ordinal scale, however, is that more quantitative (*in casu* statistical) operations are permissible with it than with a nominal scale. The latter only permits us to count what happens most frequently (the *mode* or *modes* of our universe of discourse). With an ordinal scale we may for instance also calculate some other and theoretically more fruitful types of averages.

I have wondered why the (widely known) approach used in *The Measurement of Delinquency* (which of course has its drawbacks and debatable points, like its one-sided attention towards the perpetrator of a delinquent act and some arbitrary decisions that had to be made during the construction of the measuring instrument) is not as a rule included in discussions about criminological typologies. A specialized book like the one by Ferdinand mentioned above does not discuss or only mention it at all. L.T. Wilkins does not refer to it in his *Evaluation of Penal Measures*, New York 1969, although it might easily have been brought into the discussion, and Wilkins is a criminologist with great interest in quantitative techniques.

In a recent Dutch introduction to criminology: G.P. Hoefnagels. *Beginselen van Criminologie*, Deventer 1969, which I have discussed elsewhere,[13] the *Measurement* approach is mentioned in passing as the construction of a

'severity scale' (thus with the accent on its material content, not on its methodological essence). I do not know, however, of any application of this scale in any Dutch research project, nor elsewhere for that matter by anybody but the authors themselves, although I cannot be sure about that of course.

Whatever its drawbacks, the step from rather arbitrary nominal 'typologies' (classifications) of the kind that were recently constructed by Clinard and Quinney towards an 'ordinal typology' (in strictly methodological terminology this would be a tautology) represents tremendous progress from the point of view of (applied) research. It is most likely that the emphasis on the 'seriousness' of a criminal or delinquent act implies an ethical approach that cannot in the long run be fruitful for research. However, the mere fact that it is possible to arrive at more than a nominal summing up in criminology should have evoked more excitement than it obviously did. One could have rejected the material contents, but retained the formal method of the *Measurement* approach.

Also, not recognizing it as one of the possible typological approaches is, in my opinion, a curious omission.

13. Further progress towards modern empirical science would be made if in criminology one could more or less forget the judicial classification with its discussed drawbacks, only taking the judicial information as a factual starting point (preferably after the penal administration has answered the scientific needs of a systematic coding of detected acts, if necessary parallel to the loose judicial one). Also it would mean a great step forward if one could forget about the several nominal 'typologies' with their host of implicit assumptions, and work with real (ordinal) typologies (like the one constructed by Wolfgang and Sellin) or better still with theoretical models. The latter certainly would be the most fruitful for experimental (or semi-experimental) research [14] and theory-formation.

A model is far less dependent upon (chance) observations than a typolgy. This does not include so-called analogous models of course (which are of some use in the discovery phase of scientific research, but unfit for use in validation) like the old-fashioned models that compare a society of a group to an organism, and draw all sorts of vague (and often unjustified) analogous inferences from it.

These analogous models, supposed to give a, perhaps slightly distorted or simplified, picture of the subject matter under research have their heuris-

tic value (like early models used in physics, comparing an electric current to the flow of a liquid, or atoms to billiard balls). But they invariably end up in hampering further investigation.

I have deliberately chosen examples from physics, since nowadays every layman knows that the great progress made in modern physics has been achieved by abolishing these analogous models.

A good model is one which maps the empirical facts in such a way that any *relationship* existing between its variables maps a *relationship* between the variables of the subject matter. Thus a good model is not a picture of things, but a 'picture' of the interaction between things, their functional (in the mathematical sense) or other relationship. Not the 'essence' of things, but the relationships between them are the true object of modern empirical science.

Just as in modern physics we are not so much interested in the question whether electrons are to be imagined as tiny billiard balls, or waves, or clouds, but in what happens between electrons and other particles, so in criminology we should not look for the 'essence' of crime in general, nor for 'typical' systems of crime, but for relationships existing between variables assumed to be relevant.

As an example of the essentialist school one might consider the Dutch criminologist Bianchi, who has been cited in the Preface, and probably, at least in some of his writings, another Dutch criminologist, G. Th. Kempe. These 'essentialist' authors still show the influence of ages of dogmatic religious and metaphysical thinking in the realm of what should be empirical science.

However, it is curious to note that the same 'essentialist' trend is to be found in supposedly hard-core, up-to-date empirical science. The well-known psychologist H. J. Eysenck in *Crime and Personality*, London 1964, blandly states that all crime (regardless of what is defined as such and how the selective enforcement procedures are carried out) is perpetrated by one typical kind of individual: the high scorer on a scale supposed to measure extraversion, neuroticism and psychoticism.

Although Eysenck allows for some rare, individual exceptions, and also for a group of people that may be introvert, inadequate and helpless (drifting into crime not because of antisocial impulses but because they cannot cope with ordinary life and may even seek shelter in jail or prison), the great bulk of criminality (again: regardless of its changing definition) is committed by the typical Eysenck criminal. Nor is it necessary to dif-

ferentiate between crimes like rape and a traffic misdemeanor: the same characteristics of the typical tough criminal are found upon further investigation, he says, among those who are generally considered by the public to be merely innocent victims of unfortunate circumstances, like those driving carelessly under the influence of alcohol.[15]

14. As is obvious from the foregoing discussion, criminology, like all behavioral sciences, is still very deficient in the use of good (powerful) classifications, typologies, models, and lacks empirical theories completely.

The only theoretical model that has been used fruitfully up to now is that of inferential statistics: an interpretation of a probability calculus that serves both as a heuristic device and as a validation instrument at the experimental (or often pseudo-experimental in our sciences) level of inquiry.

To arrive at the final aim of every science: theory-formation, we shall need models other than that of inferential statistics. A discussion of this topic is given in the next chapter.

NOTES

1. D.R. Cressey and D.A. Ward, *Delinquency, Crime and Social Process*, New York 1969. See especially Part III, 'The Administration of Justice as a Selective Process', pp. 119–242, containing seven chapters.
For an extensive motivational explanation of enforcement bias one should consult D.H. Chapman, *Sociology and the Stereotype of the Criminal*, London/New York 1968.
Recent Dutch examples of enforcement bias are to be found in the reports of the notorious witch-hunting with regard to the late *Provos* and their present heirs and in the attitude towards users of soft drugs, as compared to the attitude against, say, those who dangerously pollute our environment. That bias (or prejudice) is also strong among the public, became manifest when at the end of 1970 a tape-recorded voice of a man who professed to be the 'Delft murderer' was recorded on television. Reactions from the public (asked for tips that might lead to identification of the speaker) in descending order of frequency: 'The speaker must be a homosexual, a woman(!), a jew, a foreigner...' (He later proved to be a young Dutch man, married and father of a child). There is also bias in the non-enforcement direction. When several thousands of soccer fans 'occupied' Rotterdam streets and airport for hours on end to greet their favorite team that had become European champions at the beginnings of May 1970, no police were

around. But when in roughly the same period young people planted a few small trees in the streets as a silent protest against air pollution some of them were beaten and arrested for it by the police.
The traditional argument that traffic was being hindered was not raised against the soccer fans who not only hindered motorized traffic, but also made walking impossible and the landing of aircraft.
As for pornography, at least in the Netherlands, the tendency is to prosecute and convict small-scale dealers. A large, established Dutch publishing firm advertising, publishing and selling a 'pornography series' has never been brought to trial for it.

2. See for example E.W. Vaz, *Middle-Class Juvenile Delinquency*, New York 1967. (About boys only). Similar research was done by a team of criminologists at Groningen University under students at Groningen University, under direction of W. Buikhuisen, during 1969. The ratio of crimes detected and/or prosecuted to crimes confidentially admitted is (a) generally low, (b) lower for middle (or upper) class people than for lower class ones.

3. Well-known authors that heavily accentuate the reaction towards criminal acts that get known (instead of investigating how the act as such happened to be committed) are among others H.S. Becker (see his *Outsiders; Studies in the Sociology of Deviance*, New York 1963, accentuating the 'response of others'), J.I. Kitsuse (see his 'Societal Reactions to Deviant Behavior: Problems of Theory and Method', *Social Problems*, Vol. 9, Number 3 (Winter 1963), pp. 247–256, reprinted in Cressey and Ward *op. cit.*, stressing the frame of reference of those who react on deviance), E.M. Lemert (see his *Social Pathology*, New York 1951, stressing the concept of 'secondary deviation'), S. Wheeler and L.S. Cottrell, Jr. (see their *Juvenile Delinquency: Its Prevention and Control*, New York 1966, stressing the labelling process), E. Goffmann (see his *Stigma: Notes on the Management of Spoiled Identity*, Englewood Cliffs, N.J. 1963, also stressing the (stigmatising) labelling). Some of these authors will be discussed in Chapter V, 'Some Examples of Criminological Explanation.'

4. V. Aubert, 'White-Collar Crime and Social Structure', in: *American Journal of Sociology*, 58 (November 1952), pp. 263–271, reprinted in Cressey and Ward *op. cit.*

5. About 'criminal case mortality' one may still consult with profit C.C. van Vechten, 'Differential Criminal Case Mortality in Selected Jurisdictions', in: *American Sociological Review* (December 1942), 7: 833–839, reprinted in M.E. Wolfgang, et al., *The Sociology of Crime and Delinquency*, New York 1962. At the time this article was written the prosecution rate for offenses known was for e.g. rape 63,6%, for aggravated assault 26,3% (Columbia District). In Minnestoa these rates were respectively 84,8% and 33,6%.
Most interesting is the conclusion of the author that 'The most conspicuous

difference [between offenses against persons compared with offenses against property, C.I.D.] lies in the fact that ordinarily the ratio of prosecutions to property offenses is less than one to ten. The ratio of *convictions* [italics mine, C.I.D.) to prosecutions is on the other hand conspicuously high.'

As for socio-economic enforcement bias in regard to institutionalization of detected delinquents, one should consult J. F. Short, Jr. and F. I. Nye, 'Reported Behavior as a Criterion of Deviant Behavior', *Social Problems* (Winter 1957), 5: 207–213. This article has also been reprinted in M. E. Wolfgang et al., *op. cit.* A conclusion from these authors: 'Table II shows that there is little if any difference in delinquent behavior by socio-economic level, while Table I shows that a full 50 per cent of of all institutionalized boys come from the lowest socio-economic category which compares only 13 per cent of the high school population'. [under investigation, C.I.D.]. Reprint of these tables:

Table I. Delinquency by socio-economic status: Institutionalization as the criterion of delinquency

	Training school boys		High school boys	
Socio-economic status	No.	Per cent	No.	Per cent
I (Lowest)	73	50.0	112	13.3
II	48	32.9	333	39.6
III	19	13.0	282	33.5
IV (Highest)	6	4.1	114	13.6
Total	146	100.0	841	100.0
$X^2 = 117.0$	P is less than .001		C = .45	

Table II. Delinquency by socio-economic status: Reported behavior as the criterion of delinquency

	Most delinquent (Scale types 8–15)		Least delinquent (Scale types 1–7)	
Socio-economic status	No.	Per cent	No.	Per cent
I (Lowest)	42	16.0	69	12.0
II	101	38.4	233	40.4
III	91	34.6	191	33.1
IV (Highest)	29	11.0	84	14.5
Total	263	100.0	577	100.0
$X^2 = 4.2$	P is less than .30		C = .10	

6. A functional relationship is a relationship in which every variable from the domain of the function is mapped into at most one variable from the range. E.g. the function $f(x) = ax + b$ (domain defined as say all natural numbers between $-\infty$ and $+\infty$: $\{x \mid -\infty < x < +\infty\}$ in technical notation, could take on the value $x = 2$. This variable would then be mapped into the value $2a + b$ from the range, if this were defined as belonging to it.

Often it is possible to interpret functional relationships between variables as deterministic, causal relationships, and *relations*, whereby x may be mapped into more than one value from the range, as probabilistic relationships.

Now what would interest us from the point of view of criminology as a behavioral science is say the relationship (functional or otherwise) between a delinquent's act and one or more of the socio-psychological and/or biological variables that are hers or his. Whether we should call certain types of relationship 'guilt' is totally irrelevant, unless exact empirical indicators could be given for this concept.

7. The use of *prima facie* descriptive language in court that is in fact imperative language or action performing language is a most interesting thing especially for the criminologist specialized in sociology of penal law. This aspect of sociology of law should of course be based on the branch of modern philosophy that concerns itself with language analysis.

If a priest, for example, says 'I pronounce thee man and wife' he is not *describing* an action but performing the action that his words describe by uttering them. The same goes for the utterances of judges in court when they convict a person.

Now the action performing aspect of court language is usually understood by lawyers (maybe because of their training in formal law, like penal process law) but neglected by sociologists of (penal)law.

With imperative (or optative or exhortative) language that exists e.g. under the guise of a run-of-the-mill forensic psychiatric 'description' it happens to be the other way round. Lawyers seem to be inclined to 'substantialize' concepts like (in Dutch law) '*verminderde toerekeningsvatbaarheid*' (roughly: 'compulsive crime' in English) for which no empirical indicators have been given (except for vague and subjective clinical impressions of psychiatrists), nor probably can ever be given, whereas sociologists (often also a little trained in philosophy and psychology) are aware of the fact that the medical expert (often reluctantly) uses this judicially recognized concept in order to get the judge to make a decision regarding the accused that seems the most preferable to him.

The important point is that imperative, opative/exhortative and action performing language do not map any empirical reality outside the psyche of the speaker as ordinary descriptive language usually does, but that it is the kind of language that helps create a (future) reality.

Although most modern forensic psychiatrists probably would not even want to try to indicate what 'reduced accountability' maps in sofar as the past behavior of

the accused is in question, they may very much wish, by declaring the accused only 'slightly' accountable for her or his deeds, to create a host of (most of the time predictable) empirical facts.

8. See: D.R. Cressey, 'Role Theory, Differential Association and Compulsive Crimes' (especially the paragraph on a sociological theory of motivation)in: A.M. Rose (ed.), *Human Behavior and Social Processes*, Boston 1962, pp. 447–467.

9. The term 'theory' is probably the most abused one in all behavioral sciences. It may be used for practically everything that is not like digging with a spade or looking through a microscope or doing statistical calculations. However, Rudner's definitions of it seem to me quite just and in accord with the facts of theoretical systems in our ancestral and paradigmatical sciences: the physical ones.
As Rudner rightly remarks (*op. cit.* pp. 101-102): 'When invidious comparisons are made between the social and nonsocial sciences, the basis for pejorative judgments of the social sciences is not, in fact, to be found in methodology. On the whole social scientists today seem at least as methodologically aware and sophisticated as physical scientists. Nor should the basis of such judgments against the behavioral sciences be sought in the availability of observational data – the amount of reliable data now available to behavioral scientists is staggering. Clearly, much more is available than can be digested. Instead, the basis of such a pejorative judgment rests precisely on the existence of well-articulated, well-confirmed, comprehensive bodies of theory in the non-social sciences in contrast to the few such "respectable" theories in the social sciences.'
If I had preferred a formal instead of a material motto for this book, I would have chosen this quotation.

10. Rudner *op. cit.* distinguishes two major types of typologies:
1. those giving a serially (or quasi-serially) ordering of their universe of discourse;
2. those giving a metrical ordering.
For a serial ordering it is necessary that a relation-term R when applied to D (the domain or: all the elements contained in the universe of discourse) divides D into sub-sets. R must be a transitive, irreflexive, asymmetrical and connected relation. Although without a little study of modern algebra this probably will not be very clear or interesting for most readers, I shall quote Rudner's exemplification of these attributes of R (*op. cit.* p. 36, note 19):
'R is *irreflexive* relative to D if none of the things in D stands in the relation R to itself. R is *asymmetrical* relative to D if for every pair of things x and y in D of which it is true to claim that x stands in relation R to y, it is *false* to claim that y stands in R to x. R is *transitive* relative to D if for any three different entities x, y and z of D, it is true to say "If x has R to y and y has R to z, then x has R to z."'
'... Finally, R is *connected* relative to D if for any pair of members x and y of D, either x has R to y or else y has R to x.'

(Examples given by Rudner for transitivity: the relation 'taller than', for connectedness: 'older than').

Now a metrical relationship has to fulfil all of the foregoing conditions and moreover the axiom set of some number theory (e.g. the theory of natural numbers or real numbers or complex numbers) should constitute a *model* for it, which means that such an axiom set should be an alternative interpretation of the same calculus. Then and only then can we ('are we allowed', for the prerequisite is very often neglected in actual research) make fully use of quantitative measurements. This highly complex and difficult subject is more extensively treated than either in Rudner *op. cit.* or in the present book in e.g. the following two books: C.W. Churchmann and P. Ratoosh (eds.), *Measurement: Definition and Theories*, New York 1959 and in *International Encyclopedia of Unified Science*, Vol. II, Number 7, Chicago 1952 (see especially C.G. Hempel, 'Fundamentals of Concept Formation in Empirical Science'.)

11. This classification of criminal acts is elaborated in Chapter IV.

12. A ratio-scale (or metrical scale in Rudner's terminolgy) is a scale for which the axiom set of a number theory constitutes a model. (See also note 10).

13. In the Dutch *Tijdschrift voor Strafrecht*, March 1970, pp. 50–56.

14. By semi-experimental procedures I mean those that do not quite fulfil the condition that an experimental group and a matched control group are observed/measured with regard to their reaction to one stimulus, all other conditions held constant.

This stringent condition is of course never met outside the physical sciences (probably not even in biology). In practice one might regard especially 'experiments' lacking a control group as semi-experimental, or those in which intervening stimuli are obviously present and disturbing the design.

15. Eysenck's assumptions have been throughly analyzed and rejected (the latter on empirical grounds) by M.S. Hoghugi and A.R. Forrest.

See their article 'Eysenck's Theory of Criminality: An Examination with Approved School Boys', *The Brit. J. of Criminol.*, Vol. 10, July 1970, No. 3 (pp.240–254). An extensive list of References on the personality topic is added.

'According to Eysenck one should not find a fair sample of delinquents who are high on neuroticism but *not* high on extraversion, or vice versa.' the authors state. They then tested a sample of 100 delinquents and 100 matched controls on extraversion and neuroticism on the J.M.P.I. (*Junior Maudsley Personality Inventory;* Manual published by W.D. Furneaux and H.B. Gibson, London 1966).

Their conclusion is that 'The results of the studies consistently suggest that, contrary to prediction from Eysenck's theory, persistent young offenders are significantly more *introverted* than the normative samples or control groups. Furthermore, extraversion as measured by these tests, appears to be a poor correlate of difficult behaviour which is unresponsive to the normal reward and

punishment strategies used for ensuring acceptable behaviour in residential settings. It has been argued that the hypothesised tendency of offenders to greater extraversion is the cornerstone of Eysenck's theory. Therefore, it should be concluded that according to the normal criteria for acceptance of a theory to which Eysenck also subscribes' ... 'his theory of criminality has been "falsified".'

CHAPTER III

Mathematical models and methods in behavioral science

1. There is a consensus of opinion among scientists that physics is the best developed of all the empirical sciences and that of all the behavioral sciences (or social sciences in an older jargon) economics probably rates first place. The quality of these two sciences in particular is due to the formalized language which they use (the language of mathematics). The cumulative effect of their high level of abstraction, their general applicability, and the relatively rare falsification of their theories, are all achievements made after mathematical language became their primary tool.

Two very basic happenings in the material world that mathematics may describe are (*a*) a *change* in the variables under consideration, a growth, a difference in magnitude (general basic notation for this $\Delta x = x_2 - x_1$, whereby the Greek letter 'delta' may be thought of as shorthand for *difference*) and (*b*) a summation of things (general basic notation for this:

$$a_1 + a_2 + \ldots + a_{n-1} + a_n = \sum_{i=1}^{n} a_i \ ).$$

(In the latter case the Greek letter 'sigma' must be read as an instruction to add all indexed *a*'s, whatever their value, from *a* indexed 1 till *a* indexed *n*, *n* included.)

With these two very simple operations the physicist may subtract and add discrete values of his variables, or better: describe these operations very succinctly and clearly.

If the physicist's variables have not only a certain *magnitude*, but also a well-defined *direction* with respect to some frame of reference (the coordinate system under consideration), addition and subtraction are described

in vector-language. Also there are rules for the combination of pure quantities (so-called *scalars*) and *vectors* (things with both magnitude and direction). And since multiplying is only a matter of repeated addition, division of repeated subtraction, already a host of mathematical possibilities are opened up by now.

For continuous phenomena that involve the problem of *limits* (in physical practice e.g. a change of 'almost' zero in some continuous variable) infinitesimal calculus is at the physicist's disposal. But as behavioral sciences predominantly have to work with discrete phenomena (like number of persons, number of people of a certain age group and such) we shall not go into that now.

Physics and related sciences seem to be in a privileged position, because it is so easy, at least in principle, to count or measure their subject matter. And for entities that have both magnitude and direction (vectors like for instance acceleration; force) it is always easy, in principle, to choose a frame of reference.

In economics it is the discrete money and price system that makes counting easy. There is a unit of measurement because of the money and price system.

2. Now let us suppose for the sake of the argument that every behavioral scientist (*in casu* criminologist) would agree that measurement is the core of empirical science. Then it is evident that mathematics is a highly desirable formal language to describe counting and measuring with, because of its clear, unequivocal and emotionally neutral language, its axiomatic structure (see foregoing chapter), its heuristic value when some mathematical system is used as a theoretical model.

Further it is evident that not only 'discovery' (by using mathematical models) but especially 'validation' (these are the two basic scientific processes) is made much easier by mathematics because of its well defined concepts and operations.[1]

Even those who would like to discard mathematics in behavioral science as a tool of discovery (defending the necessity of specific operations like *Verstehen* through e.g. participant observation or other specific discovery approaches) or who would grant mathematics only a secondary place in the discovery phase, might agree that its value as a tool of *validation* of statements is unsurpassed.

Now if there were concensus of opinion up to this point, the great problem would be that in most behavioral sciences apart from simple counting of (frequencies) of variables (for which the natural number system serves as a theoretical model) it is relatively hard to decide how continuous phenomena (if present) should be measured. What should be the unit of measurement when we try to describe the (relationships between) behavioral variables? (We have no meterstick, no standard kilogram). Then, if we want to describe some behavioral variables as vectors (having both magnitude and direction), how should we determine a coordinate system, a frame of reference?

If the development of theory-formation (in the strict sense; see foregoing chapter) is far from satisfying in behavioral science, this may be due to two factors:

1. The resistance (mainly by an older generation who lacks the adequate training and therefore motivation) to employ mathematics of any kind (the preferred tool being ordinary language or sometimes a kind of shorthand notation for it);[2]

2. The impossibility to employ the best known parts of mathematics (calculus and analytic geometry) to our kind of phenomena, as a rule.

It is important to realise that even in the enviable sciences like physics, (theoretical) growth has not always been as rapid as during the last few centuries and decades respectively. Only after the development of calculus and analytic geometry during the seventeenth century, and the growing belief of physicists that they could use that kind of mathematics, did the kind of science that is now so powerful (both in a theoretical, an empirical and an applied way) develop.

Before the renaissance, physics was in much the same position that the behavioral sciences were in until rather recently. Then, with the great mathematical advances of the renaissance age, physicists noticed that they could use the then new kind of mathematics to describe, explain and predict physical phenomena (be it in an extremely reduced way, by neglecting every aspect of the physical world that is not in principle measurable).

3. One might wonder why, for instance, sociology did not at once follow the illustrious example of physics. This may be due to a kind of sociocultural necessity that obliges a new science to go through an everyday language phase first. It takes great intellectual effort just to detect (and defend) a new kind of scientific subject matter, so that little energy may be left over

to do more than indicate this subject matter and talk about in a programmatic way.[3]

When this first phase was over in sociology, the formalized language that empirical sciences in general need, was in fact still lacking. It is within the last few decades only that modern mathematics with some of its new branches became more generally known and that there were renewed attempts to apply mathematics (of a sort) to the behavioral sciences.[4]

The first mathematical branch to take hold of the behavioral sciences was probability calculus, on which inferential statistics is based.

At present behavioral phenomena are as a rule handled in a quantitative way with the help of statistics. Statistics is a set of techniques, based on mathematical principles, that is used to obtain, analyze and present *numerical* data. It is one way to handle (vast) amounts of numerical data. Two main branches can be distinguished:

Branche 1 is descriptive: it gives rules about how to collect, classify and condense numerical data (condensing by grouping data and/or by calculating one or more 'averages' for instance), and how to present the results in textual, tabular or graphic form.

One of its most fruitful procedures is correlation calculus: a technique by which eventually one number is calculated, the coefficient of correlation, lying between -1 and $+1$, that clearly indicates to what extent two variables are related, that is to what extent variations in one variable go with variations in the other.

Branch 2 is inferential: it gives rules about how to make inductive statements, that is: how to generalize from a (preferably random) sample to the population from which the sample was drawn, or how to test hypotheses.

This second branch is of course the more interesting one, methodologically. Its mathematical techniques are of a more subtle and complicated kind, all the epistemological problems about induction arise, and the whole is based on a probability calculus of great refinement but with room for further fundamental discussion.

By virtue of being the first branch of mathematics to become well known among behavioral scientists, inferential statistics and the probability calculus on which it is based, threatened to become sacred cows: the only kind of mathematics that was considered to be useful in the behavioral sciences.

Those behavioral scientists who objected to the use of mathematics in behavioral science, in fact mostly objected to *quantification* in behavioral

science, and in particular to the kind of quantification they knew best: descriptive and inferential statistics.

One of their (P. Sorokin; P. Winch; C. Wright Mills and others) main objections was that by reasoning about behavioral phenomena in a formalized (mathematical) way, the 'meaning' of behavioral phenomena would be lost.

The objection is in a way warranted, only it is based upon a misunderstanding about the aims of an empirical science. One might just as well say that due to an explanation of its chemical structure, the taste of the soup gets lost, or that an astrophysical description of the solar system fails to convey the emotive power of a sunset.

Scientists should never pretend that they are creating an exact duplicate of the empirical world in scientific terms. On the contrary, by extreme reductions they make a manageable universe of discourse possible. In the famous words of A. Einstein: 'It is not the function of science to *give* the taste of the soup.'[5]

The statement that nature is what physicists talk about is untrue. Physicists talk about a physical universe of discourse which is a rather quaint reduction of the empirical universe. The latter is only considered under quantitative aspects in a peculiar (and rather arbitrary) way. Behavioral scientists can do the same with their social universe if they wish to create the beginnings of a science instead of a curious sort of literature.

4. Those who object to the use of mathematics (or specifically quantification in the form of statistics) in behavioral science, generally adhere to one of the following types of reasonings:

a) Scientific statements should constitute a 'picture' of the empirical world, or should at least invoke in us the same experiences (sensations, feelings etc.) that the actual objects do.

This is what is called in epistemology the *reproductive fallacy*. It may be refuted in several ways and from several points of view. One of the typical modern refutations is to be found in S. Toulmin, *The Philosophy of Science*, London 1967 (1953[1]). In this rather elementary book on methodology the reproductive fallacy is attacked from an instrumentalists point of view.

b) Especially in behavioral science with its human objects, the failure to reproduce the rich, varied direct experience ought to be considered as disastrous.

Those who cling to this argument not only make the reproductive fallacy but add to it what might be called the uniqueness fallacy.

Whether or not human beings are unique, it is not their uniqueness that science tries to unravel, or possibly could unravel. On the contrary, the perennial task of science is to detect what things, or happenings, or people, or groups of people, or human acts have *in common*, to find the general traits or structures, the regularities, the 'laws'. (This typical task of science, in which it may be the opposite of the arts, has alrady been remarked by Aristotle).

Moreover, the combination of common elements in individual human beings may be unique, but a unique combination of unique *elements* would be impossible for our intellect to grasp.[6]

c) To grasp the meaning of behavioral phenomena very special, and certainly not quantitative methods are necessary. From M. Weber onwards who claimed the necessity of *Verstehen* ('empathy') as a specific method for behavioral science, to P .Winch, for example, (*The Idea of a Social Science*, London 1967 (1958[1]), this specific method has been thought of as a kind of 'intuition' of things or people or relationships.

Two objections can be made to this variety of what is sometimes called the 'humanistic' approach:

1. One needs a standard with which to compare one's intuition in order to know if it is right. If two or more intuitions of the same thing are contradictory (e.g. two psychiatric diagnoses of the same delinquent), the need for comparing the knowledge gained by *Verstehen* or such with some other source of knowledge becomes still more evident.

2. Even if *Verstehen* were all right as a specific method in behavioral science, its value would lie in what it had to offer in the discovery phase of scientific activity. For validation of hypothetical statements it would be useless. One would still need other methods that are more apt for intersubjective ('objective') comparison and control.

5. Contrary to the by now 'classical' mathematics (dating from the renaissance) with differential and integral calculus and analytical geometry as its main branches, modern mathematics is no longer simply the language for that which is countable and measurable.

Mathematics in the present century cannot longer be defined as such. Very important new branches like the new algebras (*plural*), set theory, topology, the theory of metrical spaces (directed graphs or graph theory)

and the newest branches like game theory, simulation theory and matrix and vector algebra when applied to *finite* sets, gave it totally new dimensions.

Modern mathematics might be defined as *an axiomatic science of abstract relations in the largest sense.* For example modern abstract algebra does not calculate with the help of certain operations, but it investigates which operations are defined in a given set, and what the relative position is of these operationally divergent sets. In the same way that high school algebra is one grade more abstract and therefore general than ordinary arithmetic, so are the modern algebras one grade more general than the kind of algebra one is taught in high school.

One branch of pure mathematics: probability calculus, and mathematical statistics that is based on it, have been used in an applied way by modern behavioral scientists *ad nauseam.* Why this is so and why there is a dichotomy among research workers in the behavioral field: either verbally qualifying or mainly using statistics (but very rarely other mathematical models), is a question for the sociology of knowledge that cannot be solved here. (How will that discipline tackle the problem: verbally or statistically, one might wonder).

Anyway, with the exception of some American work, very little interest has been shown outside economics in mathematical possibilities other than those offered by statistics.

6. What happens in behavioral, or more specifically, sociological research (of which criminology as a fundamental science is a specialized branch; see Introduction) is essentially this: a gifted researcher, observing a class of phenomena that he would like to describe in a formalized way, develops a new theoretical concept or applies an existing one. (Examples: 'anomie', 'generation gap', 'secondary deviation', 'differential association'). In general: the class of phenomena *A* (consisting of the events $a_1, a_2, a_3 \ldots a_n$) is supposed to be structurable with the help of concept *C*. (The selection of the right concept or class of concepts is the most creative moment in research work).

In order to 'prove' that it is *C* which structures *A*, indicators for *C* have to be devised that can be empirically observed and measured. In macrophysics this measuring would be done with some kind of pointer-reading, in microphysics the pointer-readings would be interpreted as indicators of the theoretical concepts. Since the pointer-readings are stan-

dard for vast classes of physical phenomena, it is quite possible that one will not find what one is looking for in this instance.

In behavioral science, however, when a concept is new and intuitively drawn from the observations made, one will always find what one is looking for when measuring one's population with the help of the indicator's chosen. As soon as the theoretical concept of e.g. 'Oedipus complex' is conceived and accepted by a researcher, Oedipus-complexes will be what he comes across when measuring his population with the 'Oedipus scale'. Although the *distribution* of 'Oedipus complexes' or their grade of severity cannot be predicted before at least a sample has been investigated in this respect, they will certainly appear to be present in some distribution and in some variety of degree. Only the researcher who considers 'Oedipus-complex' a crude and useless concept will never happen upon the phenomenon, because to him the scale measuring it is not acceptable!

This situation is quite different from that in established empirical sciences like physics, where the basic concepts (for example electromagnetism; gravitational field; gamma rays) are generally agreed upon, and the measuring of what are considered the indicators for these theoretical concepts is of a general nature, so that there will not start a discussion whether or not there is a gravitational field at location *P* nor how strong it is.

We see that what are usually called theories in behavioral science are nearly always sets of concepts (combined in statements and sometimes with the specific scales that should measure their existence and intensity given) that constitute in fact models.

These models (hardly ever fulfilling the requirements for theoretical models described in Chapter II, so maybe we should coin a special term for them like pre-models or such) are abstract mappings of data that are able to generate hypotheses, but they are essentially untestable themselves.

Merton's anomie 'theory' is really a typology that might be developed into a model with some effort. When one uses an anomie-scale one is not testing some theory or hypothesis, but investigating whether a set of data can be efficiently mapped by the typology or model that helped conceive the scale.

Of course from any good model (that is a model which is made of unequivocally defined concepts and that is free from inner contradictions) hypotheses and theories can be generated that can be tested if they are well (operationally) formulated. But how can one test them in practice?

In physics, however far-fetched the model, the same old pointer-readings

are used pervasively. But what are the equivalents of a meter, a balance, a thermometer, a galvanometer in sociology?

In sociology, and thus in criminology, what is lacking are (*a*) real theories that are not models in disguise (or typologies, or classifications), and (*b*) some method(s) of pointer-readings that are independent of the theory (or hypotheses) under investigation.

It may also be due to this gap in behavioral science that statistics took such a flight.

7. In the field of mathematics there is a mathematical system, based on probability theory, known as mathematical statistics. A mathematical system is a set of non-defined (primitive) elements. When this abstract system is interpreted in one way or another we have a mathematical *model* (abstract mapping of data). (Since mathematics is not an empirical science it is, in general, not meaningful to speak of theories here.)

Applied statistics as it is used in the empirical sciences, is in fact a mathematical model. From the model of applied statistics hypotheses concerning the empirical subject matter may be derived that can be tested with mathematical methods that are independent of the subject matter in question.

Descriptive statistics being no more than a number of ways to 'condense' numerical data, we can concentrate on inferential statistics as a model (an interpretation of an abstract mathematical system called 'mathematical statistics') and see what it can do.

Three main types of inferential statistics may be distinguished:

a) the inductive generalization from the properties of a sample to the properties of a population;

b) the inductive generalization from present properties of a sample to properties of the same sample in the past (in fact the latter are then taken as the population towards which the inductive generalization is made);

c) the inductive generalization from present properties of a sample to properties of the same sample in the future (the latter being taken again as the population towards which the inductive generalization is made).

The first kind of inference leads to what are sometimes called 'substantial hypotheses'. The second and third kind lead to explicative and predictive hypotheses respectively. The last two may of course be combined with the first type, when the explanation or prediction is also generalized to a population.

The way to test a hypothesis that is generated by the model of inferential statistics depends on what one is looking for and also on the kind and number of variables. However, any test that is applied is essentially based on a probability calculus, by which any 'event' in the sample space is mapped into an 'outcome' in the closed interval from 0 to 1 (in mathematical symbolism indicated by [0,1]), or by $\{ p \mid 0 \leq p \leq 1 \}$ where p is the value of the outcome).

What happens is that the *domain* is defined of those events that are to be considered and which will be mapped into the 'outcome' range [0,1]. The rule by which to calculate an (unique) outcome for each event is determined by the probability calculus.

Since we have a domain, a rule and a range, and for *each* element of the domain (any event e) an *unique* element p from the range, we have a mapping, whereby $p = f(e)$: a mapping with e as the independent variable, p as the dependent variable.

The difference between an ordinary mapping and a mapping generated by probability calculus lies in the kind of rule applied. Where ordinary mappings (or functions, if the mapping is from R to R) indicate ratios and differences between two (or more) variables, for example in $f(x) = y = ax + b$, probability functions are of one of the following two types:

a) In so-called *statistical probability* a value p is given to the relative frequency of an *event*. Thus a ratio is expressed between all possible outcomes (the outcome space) and the number of times e will happen (in an ideal experiment).

b) In so-called *logical probability* a value p is given to the validity of the inference itself.

In the first case one maps the outcome of an event into a value p, in the second case it is the truth-value of some inference that is considered the event and which is mapped into the range of all possible values p.

8. We are now ready to evaluate the monopolistic position of inferential statistics in behavioral science.

In physical science, when a mathematical system is interpreted in such a way that it becomes a physical model, it may be any kind of model. When hypotheses are generated by the model, they may be tested by any kind of calculus, independent of the theoretical implications of the model itself. It is only in *experiments* where no theories or no theories of a sufficient level of abstraction are available, that statistics is used.

In behavioral science anything even remotely resembling a physical theory is completely lacking. So what is used is a mathematical model suited to the very low level of abstraction that predominates in this field.

The essence of inferential statistics could be formulated in this way where it uses statistical probability: *inferential statistics aims at the comparison of obtained data (raw or grouped or otherwise processed) with chance expectations.*

If we had anything like a theory in our science of criminology, we could couch our 'expectations' in these theoretical terms and then test (by experiments or less powerful investigations) whether these expectations agreed with our statistically processed observations. This might work in two ways: either we could test whether our present observations agree with the theory, or we could make predictions, on the basis of our theory, about what will happen in the future to our data under investigation and then wait to see if this comes true in the course of time (as an astronomer would do).

However, as we said, we have nothing remotely resembling a real theory in our field. Therefore our 'expectations' are just those of statistics themselves. We expect, or pretend to expect, a normal distribution, or a binomial distribution for instance.

When our observations are in accordance with these expectations, our hypothesis is accepted: 'no significant relationship present'. When observations and expectations clash we reject our null-hypothesis and ascertain the presence of a significant difference.

At this point our theories should come into the picture and explain, but since they do not exist all we can do is note the significant difference from a chance distribution and consider the investigation completed.

The following trivial example may illustrate the above. Suppose a criminologist wants to investigate whether half of the people commit half of the crimes, that is, if everybody has the same chance of committing acts that are defined as criminal in his political community and in his day. Or more specifically: if everybody has the same chance of being detected, prosecuted and convicted for committing those acts. He will start by drawing a semi-stratified sample, that is he randomly selects e.g. 1000 men and 1000 women. His next step is to formulate a hypothesis H_0 about some property or class of properties of the two sample strata. In this case let H_0 be: 'the incidence of convictions for crimes among the randomly selected males will be equal to that among the females.' Or to be more exact: any difference in this respect among the two strata above a certain conventionally selected level will be considered 'significant' and will lead to the rejection of H_0.

In formalized notation:

$$P(A \mid H_0) = 0{,}50$$
$$c = 0{,}05$$

This can be read as: the probability (P) of the event A under H_0 is one-half; the confidence interval chosen is of a magnitude of 5%. (If we find A in no more or less than half of the cases plus or minus 5%, this will be ascribed to the normal chance of having drawn a slightly biased sample).

In the trivial example given above, the criminologist will find discrepancies between male and female convicts far greater than 55% versus 45%. Therefore he will reject H_0 and conclude that there is a significant correlation between the variable *(being) male* and the variable *conviction.*

Of course the example given above is not meant to suggest that criminology would not be more sophisticated than that, both in its conceptualization and its statistical techniques. It is only meant to lay bare, by extreme simplification, the basic structure of a lot of statistical reasoning that goes on, although both the material contents and the techniques used may be subtle and complex.

Again, if we had anything like theories in behavioral science, we would try to subsume a new statistical finding under it, or at least we would try to create theories according for it. But in practice, after the enumeration of significant relationships between variables that have been found, behavioral research and especially criminological research usually stops. One important correlation has been demonstrated, so on to the next.

This hypothetical bit of criminological research that has been given as a structural example would, for all practical purposes, end like this: 'I found a deviation from chance expectations (based on probability calculus) which I shall consider to be significant (not due to sampling variability). This forces me to accept my hypothesis H_1: 'the probability of observing more males – or x % more males – than females in a population of convicts is 95.'

Formalized:

$$P(A \mid H_1) = 0{,}95$$
$$c = 0{,}05$$

Finis.

Note that the rejection of H_0 and the acceptance of H_1 do not imply any theoretical concepts (apart from the number concept as such, which is trivial).

If we had any theory-loaded hypothesis at our disposal (a hypothesis of explanatory character), we could start with the core of our scientific activity right where most behavioral (i.e. criminological) research stops. (We have of course explanations of sorts in the verbal systems that often are called theories, but there seldom is made a connection between them and the straightforward, hard-core empirical research that we have in mind here.)

Let us assume for the moment that the criminologist who conducted the male-female research, adhered to the pure science concept of criminology as expounded in the Preface. He would in that case generally consider the variable criminal (convict) as dependent of the combined independent variables criminal law and penal policy.

The independent variables are there as a 'cause' for the whole population, but only those persons for whom a certain combination of boundary conditions is present will manifest its effect. With the help of statistics we can detect many boundary conditions, and with either factor analysis or latent structure analysis it is even possible to determine their associations and relative weight within a certain sample under investigation. However, what weight we should assign to the several boundary conditions in general cannot be detected statistically but should be investigated in another manner.

9. Of course inferential statistics can do more than test hypotheses about the distribution of variable-values in a sample, as was shown above.

By more or less complicated, and more or less technical means, inferential statistics can make quite usefull guesses about for example the mean, median and mode or the *central tendency* of some variable in the population, on the basis of information drawn from a (often surprisingly small) sample. It can also indicate by what percentage its guesses may be maximally wrong, that is state a *confidence interval.*

In the same way inferential statistics can indicate the range, variance and standard deviation, that is the *variability* of some variable-value in a population. Further it can indicate *correlations* between two or several population variables.

All this, plus additional advanced techniques, make it a very useful tool when we are interested in the properties of some big, difficult or impossible to investigate population, from which we are able nevertheless to draw a random sample.

Furthermore, these techniques make it possible to extrapolate variable-values towards the future or towards the past.

In those situations in behavioral research which are comparable to experimental research in natural science, statistics is an indispensable tool. Its limitations are:

a) it cannot lead to abstract theory-formation by itself (although its findings may induce the creative moment of theory-formation);

b) in its most abstract, most general form, where it is 'almost' a modest theory about *in casu* behavior (e.g. that the thing to expect for some social act is a normal distribution within the population), it is restricted by its mathematical foundation (probability calculus), in the sense that it will lead only to statements that describe an inductive subsumption under laws of probabilistic form (stochastic or probabilistic in the narrow sense).

10. A stochastic process is a process in which the transition from one state to the next contains a probabilistic, random element. A good example of such a process is the casting of (unbiased) dice or flipping a (fair) coin. Although the process of casting and the outcome of it are in fact uniquely determined, the number of variables that enter into the picture is too great, and their interaction is too complex, for the human intellect ever to be able to grasp them. Therefore we consider such a process 'as if' it were totally unpredictable, even though a superhuman, omniscient mind would be able to predict the outcome in each individual case. (In fact, people claiming to possess extrasensory perception ought to be able to do so).

Most people, be it as laymen, have read about the stochastic processes in sub-atomic physics or microphysics. From these quarters a rather confused philosophical debate has ensued about whether the methodologically stochastic approach reflects systems which are stochastic *in se* or systems which are ontologically deterministic. I fail to see how one could ever prove one or the other, nor do I believe that these ontological questions need bother us very much at the methodological level. Science as a product consists of linguistic systems of one kind or another. What its subject matter 'really' is like outside these linguistic systems is a problem that falls outside the scope of empirical science itself.[7]

Stochastic processes consist of stochastic variables. When I hit a perfect billiard ball with a cue, given the force applied and the angle under which it is applied, given the friction of the surface over which the ball rolls, then I

can predict the exact velocity (speed + direction) of the billiard ball after the hit. I can also compute its (negative) acceleration, and when and where it will come to rest. In the realm of macromechanics the future of the systems under consideration is known. Deterministic predictions can be made and even if these prove false sometimes (because of intervening variables which it is not possible to ignore or control entirely), this does not make them less adequate. Often physicists can analyze why their (ideal) predictions proved false in a particular case.

Stochastic variables on the other hand are those whose particular values cannot be predicted by even the most brilliant human intellect. The chances for any of the future alternatives are known but not which outcome will happen in a particular case. Most systems in behavioral science have to be considered as consisting of stochastic variables. The common example for such a variable in the physical world is the flipping of a fair coin. It is impossible to predict whether heads or tails will come up, even though the eight (or n) preceding throws may have all come up *heads*. (The coin has no memory, so if it is really fair chances are fifty-fifty for every throw no matter what the outcomes before.)

A stochastic variable is thus one whose particular values are unpredictable. With the help of probability calculus, however, I know what the distribution of all possible outcomes will be in the long run, although I cannot predict the outcome of any particular event.

But if I flip a fair coin several times, each side of the coin will tend to appear in one half or 50% of the cases. The more flips, the less the observed distribution of the outcomes will deviate from the expected distribution.

In casting a die, each side of the die will tend to come up in one sixth of the cases.

In many instances where we have to deal with stochastic variables, probability calculus can help us to make an exact prediction for a whole range of outcomes (e.g. the distribution of the outcomes in the long run, or the way in which a variable will be distributed in a population), but it cannot predict anything exactly for individual outcomes. It can again predict what maximum percentage of the range prediction may be wrong and to what maximum degree, that is, with probability calculus one can predict the maximum number and the maximum value of one's errors in predicting.

It is important to remark again at this point that it is not necessarily the case that probabilistic statements describe indeterministic (or stochastic)

ontological systems, just like deductive nomological statements do not necessarily describe deterministic systems.

What our universe of discourse looks like at the ontological level (or better: what model we want to apply to it) is to be distinguished sharply from the theoretical statements that we use to describe the system.

The importance of this distinction becomes clear if one realizes that in behavioral science we may describe election behavior or behavior on parole as stochastic variables. We cannot predict how Mrs. *A* will vote on election day but we can often predict how a whole nation will vote, as well as how big the maximum error of our prediction will be. (If even the predicted boundaries of the error prove wrong, one should have chosen a lower level of information, and used a gametheoretic model, that is a model suited for systems of which only the future alternatives are known but not their relative chances of being realized).

In criminology the behavior of Mr. *X* who is on parole is considered a stochastic variable. His behavior may be entirely determined by external and internal causes, but these causes are far too numerous and their interaction too complex for us to ever know, just as we cannot know everything that plays a role in flipping a coin or casting a die (muscle tension, air resistance and so on). But although we cannot predict deterministically how Mr. *X* will behave at time *t*, we can predict what his behavior will probably be like in the long run (here the set of all his future acts is the range) or we can predict what the probable behavior will be of the set of parolees of which Mr. *X* is a member. In either case we can also predict the maximum error in our prediction.

11. From this informal introduction to what is meant by stochastic processes or stochastic variables, we come to the following somewhat more rigorous mathematical presentation:

Def. 1: A stochastic process is a set of time-indexed stochastic variables, whereby each possible outcome is mapped into just one value from a range of real numbers.
A stochastic process is therefore to be considered as a function, mapping a set of time-dependent outcomes into a set of values for their realization. Or:

Def. 2: A stochastic process is a time dependent probability process.

It is usual to classify stochastic processes into four main categories, ac-

cording to whether the time-process is considered as discrete or continuous (a series of observations during an experimental situation is a discrete time-process; non-experimental situations in which the variable under investigation is supposed to be continually present, whether observed or not, can be considered continuous time-processes), and according to whether the realized state is considered discrete or continuous.

(In behavioral science the space of realized outcomes will be discrete more often than continuous, e.g. when the final states are nominally classified – like recidivist – non-recidivist – or when their frequencies are counted).

The four categories are:

1 continuous time; discrete (realized) state
2. continuous time; continuous state
3. discrete time; discrete state
4. discrete time; continuous state

Category 3 is important in experimental situations in behavioral science, category 1 in non-experimental situations, while categories 2 and 4 are seldom relevant.

A step-function of any kind is an example of category 1. Branching processes or, more generally, evolutionary processes are examples of category 4.

In applied criminology the behavior of persons on parole can be considered a stochastic process of type 1. The spreading of delinquent norms in a community of convicts can be considered a stochastic process of type 4.

12. What does it mean for theory-formation in criminology that if variables are measured and if a mathematical system is used as a model, it nearly always happens to be statistics that is used?

The fact that statistics is based on a probability calculus, that it is concerned with stochastic experiments (experiments which, when replicated, will yield at least two outcomes; if one outcome only, the experiment is determinate) does not in itself mean that theory-formation in this particular branch of behavioral science is hampered.

Of course one gets the most definite information concerning the future of the system under consideration if it is possible to use a deterministic model. This is almost a tautology, because the applicability of a deterministic model means that perfect prediction is possible, that the future of the system (given constant boundary conditions) is known. On the other

hand if one applies a probabilistic model, it means that only the chances for the future alternatives are predictable. A still lower level of information is that of gametheoretic models, yielding only the future alternatives, without relative weights. But whatever level of information is reachable through whatever type of model, it does not mean that statistics only should be applied.

Statistics is the ideal tool at the experimental level of science. Its results, however, should be interpreted with the help of more powerful models, which may be stochastic too (e.g. Markov chain models) or deterministic (e.g. a deterministic model derived from mathematical graph theory.) By experimental or semi-experimental investigations, described in statistical terms, we may detect certain uniformities or regularities. Theories should then come into the picture and explain why these regularities exist and predict new regularities. Preferably these theories should be themselves not vague verbal systems, but interpretations of some mathematical model.

In behavioral science one usually stops short the moment the 'experimental' phase is finished and the interpretative phase should start. After a problem has been stated and statistical research has brought some basic order into it, a mathematical model should be chosen, however, from which consequences can be drawn in a purely deductive way. These consequences should be formulated as *hypotheses* with respect to the empirical data (already statistically processed). These hypotheses should be tested, again with the help of inferential statistics, and if the tests do not falsify the hypotheses (or make us reject the null-hypotheses matching it), we should consider the model fit for generating theories in this particular field.

Mathematical statistics is of course also a mathematical system that can be interpreted as a model, but its level of abstraction is rather low, and often the final results it leads us to are still highly ambiguous unless interpreted with the help of a more powerful (more general, more abstract) model.

In the terminology of R. E. Kirk[8] a statistical model is 'A mathematical statement concerning the sampling distribution of random variables that is used in evaluating the outcome of an experiment or in predicting the outcome of future replications of an experiment.' Of the implicit limitations of this model many (continental) criminologists seem to be insufficiently aware, whereas those that severely criticize it mostly do this for the wrong reason: fear or disgust of quantification.

13. From a strictly methodological point of view (general methodology with regard to any behavioral science) sound practical advice has been given by A. D. de Groot.[9] 'The great importance of prestated *confirmation criteria*' ... 'will have become clear by now. Scientific usage requires that the investigator, before starting his investigative procedure, tie himself down to definite criteria for the confirmation and disconfirmation of the hypothesis he intends to test. Thus, on the one hand, he will not be tempted to gloss over, or "dress up", his results afterwards, while on the other, the outcome of the entire investigation is molded into a verifiable prediction' ...

And again: 'Whenever scientific verificatory procedures are to be carried out in practice, it is of the utmost importance that the investigator make *an advance appraisal of the potential confirmation value* of the outcome of the prediction which he is about to verify' ...

If this advice were followed in criminological research at the (semi-) experimental/statistical level, it would already mean a great advancement. In practice, not only does research very often stop short at the exploratory/-descriptive or, at best, (semi-)experimental/statistical level, but in the latter case it is quite common that hypotheses and the criteria for confirmation/-rejection are formulated *after* the statistical analysis has been done.

In the case of real theories confirmation and rejection are something different. One will hardly reject a theory because some hypotheses derived from it have to be rejected (see De Groot *op. cit.*, pp. 107–113).

The peculiar habit of not indicating in advance one's criteria of rejection/confirmation at the statistical level may be seen as a further indication that many behavioral scientists, *in casu* criminologists, tend to regard the statistical model as a full-fledged formal theory in its own right.

NOTES

1. It has been argued by many sociologists in the early decades of our century (by much fewer prominent ones now, but again by present-day politicologists, that is scientists in a rather recent behavioral specialism) that behavioral science has no use for statistics, mathematics in general, or any of the more formal approaches that are fruitfully employed in the much older (physical) sciences.
The argument is founded on a confusion between the two basic aspects of scientific activity: *discovery* and *validation* (or justification).
The former is indeed a rather unique process, distinguishing not only the behavioral

sciences from the physical ones, but also the two groups of sciences among themselves. The approach of the field biologist is quite distinct from that of his colleague using mainly the microscope, the microscope requires a different technique and mental approach from the cyclotron or the stethoscope, and all these are quite different from the one-way mirror sometimes used in experimental social psychology or from the computer used for simulation. Besides these differences in instruments used for the sake of discovery, there may be a quite distinct mental approach. Let us assume that *Verstehen* ('empathy') through, say, participant observation, is a legitimate method of discovery in sociology. That method is indeed far removed from the experimental approach. Yet, once the discovery phase is passed, the scientist is supposed to formulate and validate hypotheses derived from it. Now the logic of validation is one and the same in all the sciences, and there can be no sensible reason to avoid the language most apt for it: that of mathematics.

If one's training or inclination make it unpleasant and probably fruitless to use this language, one can very well specialize in the discovery kind of activity. Yet, in that case one should cooperate with others differently trained and motivated, to create a full-fledged science. There is no reason to drag on discussions that suggest that discovery and validation would be different schools of thought.

This topic has been very lucidly discussed, it seems to me, by R.S. Rudner in his *Philosophy of Social Science*, Englewood Cliffs, N.J. 1966 (pp. 5–7). A psychological complication, not mentioned by him, is that in practice the anti-mathematization people happen to be those concerned with major social problems as a rule, the formalists often happen to be of a conservative social bend, quite content if they can prove something, no matter how unimportant or trivial from an ethical point of view. This, however, is a psychologically interesting fact, that has nothing to do with methodology as such.

Although the present book is more in favor of mathematical models as a means of discovery than what Rudner calls the 'separatists' could possibly agree with, their worth in validating one's hypotheses seems to me undiscutable and their help in the phase of theoretical interpretation a mere matter of efficiency.

2. The shorthand type of symbolism that is only at a superficial look like mathematics, but that in reality lacks any axiomatic structure and the possibility to derive theorems from it, is for example amply used in Th. Geiger, *Vorstudien zu einer Soziologie des Rechts*, Neuwied am Rhein und Berlin 1964, an otherwise excellent book. See for example pp. 96–101.

For axiomatic systems see Rudner, *op. cit.* pp. 15–18.

3. The programmatic approach is still predominant after half a century or more in sociology of law, to the detriment of criminology as a fundamental science.

4. See for example the following books: Ch.M. Bonjean, et al., *Sociological Measurement: an Inventory or Scales and Indices*, New York 1967.

R. Boudon, *L'Analyse mathématique des faits sociaux*, Paris 1967.
H.M. Blalock, Jr., *Theory Construction: from Verbal to Mathematical Formulations*, Englewood Cliffs, N.J. 1969.
Also the article of M.E. Wolfgang and H.A. Smith, 'Mathematical Models in Criminology,' *Int. Soc. Sci. J.*, 1966, Vol. XVIII, No. 2 (pp. 220–223).
For bibliographical information see: J. Holland and M.D. Steuer, *Mathematical Sociology: a Selective Annotated Bibliography*, London 1969.
Rudner, *op. cit.* p. 69. See also p. 83 where the reproductive fallacy is discussed again.

6. For a discussion of this topic see Rudner *op. cit.*, pp. 70–71.

7. The necessity of sharply distinguishing ontological systems and the linguistic systems describing them will be treated again in the next chapter when the problem of teleological explanation is discussed.

8. R.E. Kirk, *Experimental Design Procedures for the Behavioral Sciences*, Belmont, Calif. 1968, p. 556.

9. A.D. de Groot, *Methodology: Foundations of Inference and Research in the Behavioral Sciences*, The Hague/Paris 1969, pp. 103–105.

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CHAPTER IV

Explanation and prediction

1. From a methodological point of view there is, at the theoretical level of any science, no difference between an explanation and a prediction but the psychological one for the scientist. The logical structure of these verbal statements is exactly the same, and will basically look like this:

For a statement E, describing a particular event, and a set of statements $C_1 \ldots C_n$ describing the specific circumstances that are relevant to it, there obtains a set of statements $L_1 \ldots L_n$, that are called 'laws' or abstract universal generalizations.

The statements $L_1 \ldots L_n$ imply that whenever circumstances of the type $C_1 \ldots C_n$ are present, event E will occur.

Schematically we have the following logical deduction:

$$\left.\begin{array}{l} L_1 \ldots L_n \text{ (General Laws)} \\ C_1 \ldots C_n \text{ (Statements of antecedent condition)} \end{array}\right\} \text{(Explanans)}$$
$$E \text{ (Explanandum)}$$

That is to say: E is dependent on both the L_i statements and the C_i statements, which should be true in order to constitute a valid explanation.

This type of explanation, which C. G. Hempel calls the deductive-nomological type,[1] is logically not dependent on the time-dimension, although to the scientist this dimension may be psychologically and pragmatically important.

In the case of explanation, E is an event that has passed already, and the task of the scientist is to find the L_i and C_i under which to subsume it. In case of prediction the scientist knows already the relevant L_i and C_i and is trying to establish the right E that belongs to it. This means, in the words of R.S. Rudner[2] '*that we have an explanation for an event if, and only if*

(from a different temporal vantage point) *we could have predicted it.*'

In behavioral science and especially in criminology, we seldom have at our disposal the deductive-nomological type of lawlike statements. Most of the time we cannot even hope to reach the level of explanation that consists of deductive subsumption under laws of universal form, but have to rely on inductive subsumptions under laws of probabilistic form. Whereas a law of universal form 'is basically a statement to the effect that in *all* cases where conditions of kind F are realized, conditions of kind G are realized as well; a law of probabilistic form asserts, basically, that under certain conditions, constituting the performance of a random experiment R, a certain kind of outcome will occur in a specified percentage of cases'.[3]

Although logically of a different character, the latter type of laws have just as well explanatory power. They have been used in genetics, statistical mechanics and quantum theory with great success.

It should be noted that the difference between hypotheses and theories is mainly in their degree of complexity, but that both are essentially testable and therefore in principle rejectable.

Explanation in criminology may be aimed at either through explanatory hypotheses like 'the excess of aggressive delinquency in urban slums is probably due to X' (where X may be either a single factor like, say, overcrowding, or a set of several factors considered to contain the independent variables) or through what are commonly called 'theories'. In the next chapter a few of these so-called theories will be examined in some detail: Sutherland's 'theory' of differential association, Merton's anomie 'theory', the interactionist 'theory' of which Becker is an adherent and on which Cressey and Ward based a criminological reader,[4] the 'theory' of subcultures as propounded by A.K. Cohen, Cloward and Ohlin, the scapegoat 'theory' (a specialized version of the 'labeling theory') in the form Chapman gave it, the 'theory' of secondary deviation as proposed by Lemert.[5] All these verbal systems, although they are structurally very different among themselves and although none of them is a theory in the strict methodological sense, aim at being explanatory and predictive.

What is relevant here is that for a scientific statement (hypothesis) or interrelated set of statements (theory) to be acceptable, it should be testable in principle. This depends on two things: the definition and operationality of the concepts used and the falsifiability of the statements made with them. Both conditions are independent of either a deterministic or a probabilistic formulation.

What is called explanation in criminology is often no more than a psychological elucidation, obtained by ordering data with some verbal model (e.g. the Freudian one). The difference between real explanation and the latter is that the latter offers a kind of psychological elucidation consisting of statements that are untestable *in se* (either because the concepts it uses are not well-defined or because the statements are unfalsifiable or both). If it is said that a delinquent acts in such and such a way because of 'anal regression', this statement may give people the subjective feeling that something has been 'explained'. Methodologically, however, this is not the case.

2. In behavioral science explanation is seldom given in a neutral functional way, but mostly with the help of a causal model. The concept of 'cause' is a highly complex, easily misleading and often dangerously anthropomorphic one. For these reasons, in modern physical science, one will seldom encounter the term 'cause'. Instead, functional language is used. Suppose we have a well-defined set in the universe of real numbers, A, and another set, B. Now if to each member of $A = \{ a_1, a_2, \ldots a_n \}$ there is ascribed just one member of $B = \{ b_1, b_2, \ldots b_n \}$, or if each member of *domain A* is mapped into no more than one member of *range B*, we have by definition a *function*.

If more than one member of B is ascribed to any member of A we have a *relation* only (for example the equation of a circle, in which two values of y from the range of all y are ascribed to any value from the domain of all x).

If for any member of the domain of a function there is *no* member, of the range ascribed to it, we say that the function is 'not defined' (or discontinuous) for that particular value. For example if $f(x) = \frac{3}{x}$, the function is not defined for the value $x = 0$ from the domain. It is tacitly agreed here that the complete formula should be: $\{(x, f(x)) \mid f(x) = \frac{3}{x} \text{ and } -\infty < x < \infty \}$.

To be read as: the set of all ordered pairs $(x, f(x))$, such that $f(x) = \frac{3}{x}$ and x is a member of the set of all real numbers.

Since dividing by zero is not defined in the set of real numbers, we can add 'and $x \neq 0$', but we can also tacitly assume this.

If every member of A is mapped into a member of B, but not all members of B get paired to a member of A by the function rule, we say that A is mapped *into B*. If there is an exact one-to-one correspondence between A and B we say that A is mapped *onto* B.

If several members of A get the same member of B ascribed to them, such

as for example f(3) = 12, but f(4) = 12 also, we still have what is defined as a function, since the condition was that no two ordered pairs should have the same *first* element.

The whole theory of functions is of course a subject in itself, that does not need to be treated here (we shall even ignore simple operations like the composition of functions); but what concerns us is this: 'functions' are an abstract mathematical system (like mathematical statistics), that is a set of non-defined primitive elements + operations defined for these elements + postulates and definitions.

When this abstract system is interpreted in some way, we have a theory or theoretical model (abstract mapping of data) that is suitable for interpretation as a *causal* model. If (simplified) the movement of the billiard ball can be described as a function of the impetus from the cue; the impetus may be interpreted as the 'cause', the movement as its' 'effect', if two additional conditions are met that will be discussed below. With the help of the function rule we can predict all kinds of future effects if the necessary vectorial and scalar attributes of the cause are known.

In this instance there would be a universal rule, with whose help we can predict (explain) all kinds of individual cases. The rule in question is, moreover, independent from the time factor (in contrast to stochastic functions). A *deterministic* model is available in this case.

But whenever the situation is more complicated than in the case of the billiard ball, physicists may easily avoid the dangers and problems of causal language. They just stick to (deterministic or stochastic) functional language.

In behavioral science, and especially in criminology, it is not only impossible most of the time to apply a deterministic model, but even stochastic quantification may seem hard to arrive at. In that case qualitative statements may be used that are in fact of a functional type. If a criminologist contended that say poverty, or poverty + broken homes, were the cause of crime (of such and such a type), he would in fact be saying that criminal behavior of type such and such is dependent on the independent variable poverty (or the independent variables, poverty + broken homes), but that he could not possibly quantify these concepts and their relationship.

Just because these functional statements remain verbal, one would easily be tempted to start speaking of the 'cause' (or causes) of crime, instead of the independent variable(s) for criminal behavior.

However, to do so lawfully, three conditions should be met:

a) there must be an association between the supposed cause (or causes) and the effect. This may be a deterministic association expressed through a function or a relation, or a stochastic one expressed through e.g. a correlation coefficient.

b) the independent variable(s) should be causally prior to the dependent variable: the so-called causal order should be established.

c) the original association ought not to disappear when the influences of other variables that are causally prior to both the dependent and the independent ones are removed: lack of spuriousness should be established.

These three criteria stem originally from H. Hyman and have been adopted by T. Hirschi and H.C. Selvin in their study of false criteria of causality in delinquency.[6]

Especially the third criterion is very hard to establish in criminology. Therefore it would be advisable to avoid the concept of 'cause' as much as possible. As soon as association of some kind is proved to exist between variables and the temporal order has been demonstrated, one can lawfully consider this an explanation (or prediction) of the event under investigation.

It may be superfluous to add that explanation as such has nothing to do with 'a reduction to the familiar', as one might naively suppose. For a set of statements to be an explanation (or a prediction) in the methodological sense, the only requirement is that they should be related according to the schema of paragraph 1 of this chapter. If the statements are intended to give a causal explanation, all three conditions specified originally by Hyman should be met.

3. Prediction in general is a many-valued term, which has at least five different connotations. What is called prediction in criminology is in practice restricted to statistical prediction and is, curiously enough, often treated as if it were a school of thought that one might adhere to or fight.[7] It will become clear why this is absurd if we indicate briefly what different meanings prediction can have, thereby making more clear the position of statistical prediction.

a) By prediction one might mean un- or prescientific forecasting of the future: *intuitive prognoses*. We are acquainted with this kind of prognostication in the form of fortune-telling by astrologists and in general by people claiming to possess extrasensory perception. Also: people not claiming any extraordinary perception, may simply extrapolate present facts and trends

towards the future, thereby making prognoses. According to Popper the latter type of 'prediction' is, on a large scale, unfeasible. Analysis of the occult type of 'prediction' shows that its propositions are generally vague, unclear, ambiguous, so that falsification will be impossible, regardless of what happens. Neither type of what it would be better to call prognosis is relevant for criminology as a fundamental science.

b) *Clinical prediction.* This might be an intuitive prognosis of the kind already mentioned under *a*, but in its more serious form it is a kind of subconscious statistical reasoning whereby the clinician makes inferences on the basis of his experience, that is, his knowledge about a (highly biased) sample. Since the people about whom the clinician is making predictions generally do belong to the same class from which his biased sample was drawn, his predictions may seem reasonably right.

Forensic psychiatrists may easily predict that sex offenders brought to their attention will be highly neurotic, regardless of what happens to be defined as a sex offense in their political community and at that time. They cannot predict sex offenses as a criterion from the predictor 'neuroticism', but predicting that any sex offender brought to their attention will be a neurotic would be a good strategy.

For criminology as a fundamental science clinical prediction is irrelevant because of its lack of generality, its lack of precision (no control group, no formal testing procedures, and least of all operationally defined terms), its one-sided attention towards behavior that happens to be defined as criminal, and because of its lack of insight into the legal defining process as such and the interaction between the latter and the perpetration of a crime.

In applied criminology clinical predictions should be compared by precise statistical tests with the predictive power of statistical prediction tables constructed by criminologists. The chance that clinical predictions will prove (in their success-failure rate) to be just as good or better than the prediction tables we now have at our disposal does not seem small. The resistance of many clinicians, especially those of psychoanalytical bent, to hard-core empirical research therefore seems (at least at present) premature.

c) *Statistical prediction.* This kind of prediction is what we are concerned with in present-day criminology. Current prediction tables are meant to serve as a rational decision instrument for judges and social workers dealing with criminals. Therefore what is called criminological prediction

in applied criminology is *statistical* prediction. We should not attach any fundamental or theoretical value to it.

Statistical prediction in the narrow sense of the term is achieved by regression analysis. This technique, however, has seldom been applied as yet in criminology. What is usually called statistical prediction in applied criminology is, in reality, an *estimation* technique.

Both kinds of statistical prediction and their practical application will be discussed in the next section.

d) Model-based prediction. Models have been defined as alternative interpretations of a calculus that offer us abstract mappings of data. Although they should be internally consistent, there is an arbitrary element in them and the statements they consist of though rejectable, are not falsifiable *in se*.

This implies that once a certain model is accepted as useful because of its descriptive, explanative and/or predictive power, we will discard it only for a more efficient one.

Hypotheses can be derived from so-called predictive models. The prediction of the model relies on the fact that it makes two claims: 1. that the model will be able to map future data in the same field as well, and 2. that the predictive hypotheses which can be derived from it are testable.

Methodologically it is necessary to make a distinction between verbal models mapping empirical data, on one hand, and mathematical models mapping verbal systems, on the other.

e) Theoretical prediction. By theoretical prediction we mean statements based on valid interpretation of empirically tested hypotheses which in turn are derived from a model (which may or may not have a theoretical foundation itself).

The term 'theory' and its derivatives when used in this way are the opposite of free speculation, and are essentially distinct from 'model' because of their empirical foundation.

As has already been mentioned in the first paragraph of this chapter, at the theoretical level of any science there is no logical distinction between explanation and prediction, nor does the term 'prediction' imply any prognostics. The (falsifiable) statement that is made in both explanation and prediction at the theoretical level is that for the event E, geven the circumstances (or boundary conditions) $\Sigma\ C_1$, the abstract generalization $\Sigma\ L_1$ will hold.

Falsification may occur in two ways. If a law does not hold in a particular

instance E_1, we shall try to prove that the event was incorrectly thought to be subsumable under the lawlike statements, because the necessary boundary conditions did not obtain.

The connection between event and 'law' is not falsified, but the classification of a particular event under the law is proved to be incorrect. (By redefining the necessary boundary conditions the continuity of classical physics was assured when quantum physics and the theory of relativity were developed. The old laws are still valid for all practical purposes, but only under 'moderate' circumstances).

However, if we cannot prove or assume that the circumstances or boundary conditions deviated from what they should be with regard to the instances *E* assumed to be explained/predicted by the lawlike statements, we would eventually consider these lawlike statements themselves falsified. In highly developed sciences this seldom happens because of the high level of abstraction of their laws.

In any case it is important to distinguish clearly between the irrefutability of models (except for pragmatical reasons), and the *scarce* falsification of theoretical statements.

4. What is called prediction in criminology is almost exclusively statistical prediction. In 1965, W. H. Nagel published an almost complete overview of the prediction literature in criminology up to that time. From it we see that criminological prediction, from the first scientific attempts by E. W. Burgess in 1928[8] down to the most recent attempts has been exclusively oriented towards the construction of a rational decision instrument in *applied* criminology. By giving weights to a set of predictor variables selected from the total set of information one tries to predict the criterion, which might be recidivism or future delinquency of persons who are at present non-delinquents.

It is important to notice the essential difference between the two criteria that are predicted in this type of applied criminological research. According to L. T. Wilkins[9]: 'The first attempts to predict recidivism, and the tables produced, were assumed to be of value in providing guidance for the parole board's decision in cases of indeterminate sentences. The second class of prediction methods is those that attempt to predict the likelihood that a young person (such as a child who has been found guilty of an offense) may at some future time become an adjudicated delinquent or criminal.

'The similarity between the two classes of prediction is superficial, for there are considerable differences in the required methodology and seriously different problems in relation to the moral questions involved. These studies would better be designated as experience tables, or tables of base expectancy, or perhaps even more appropiately, estimates of the probability of recidivism, rather than prediction.'

We can roughly call this former kind of statistical 'prediction' in applied criminology *estimation*, whereas what is generally called prediction in statistical language is (multiple) regression analysis.

The latter, however, is seldom applied in criminology for the following reason: when applying the statistical estimation techniques, one is drawing inferences from sample statistics (or *estimators* that have been observed or measured) towards values that are descriptive of the population, generally called *parameters*.

If, in criminology, one has observed (measured) estimators for one individual, one can consider his future behavior to represent the population for which inferences are drawn. If one has observed estimators for a group of delinquents, one can draw inferences for all delinquents (preferably those one considers to belong to the same category), thereby considering (the class of) all delinquents (of the same category) as the relevant population.

If one supposes, as the Glueck's evidently do, that 'delinquency', alternatively 'non-delinquency', is an essential category of behavior, one could also try to draw the same kind of inferences for non-delinquents.

In regression analysis the procedure is exactly the opposite, insofar as the regression equation(s) represent(s) our knowledge about relationships between variables for a population. By concluding that a particular relationship belongs to the class for which the regression equation(s) hold(s), we can make predictions for that particular relationship by substituting the particular value(s) of the independent predictor variable(s) and predicting the corresponding dependent variable (which we can now calculate).

If we had measured continuous variables among which a perfect functional relationship (every value from the domain mapped into just one value from the range) existed, we could represent this – in the simplest case of a linear functional relationship between *two* variables – by a simple function of the type

$$Y = aX + b$$

in which the coefficient *a* represents the slope of the straight line that is the graphical representation of this function, and the constant *b* the so-called

Y-intercept. (If $b = 0$ the line goes through the origin (0,0); if $a = 0$ the function is represented by a straight line parallel to the X-axis or 'ordinate'; if $Y = 0$ the line is parallel to the Y-axis or 'abscis').

Figure 1

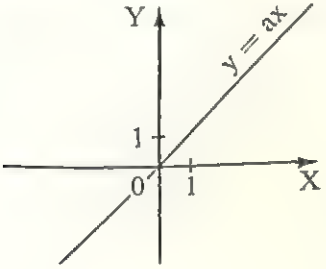


Figure 2

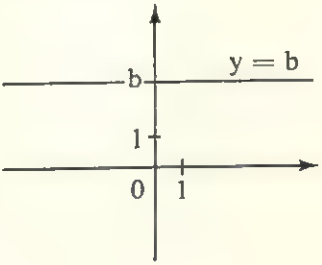
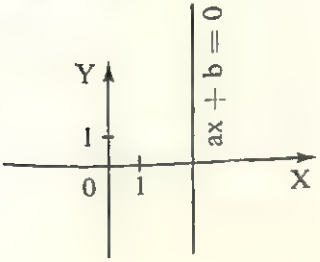


Figure 3



In general functions of a higher order (in which X is present with a higher exponent than 1) will be written as

$$Y = a_1X^n + a_2X^{n-1} + a_3X^{n-2} + \ldots + a_{n-1}X + a_n$$

if there is a functional relationship between only two variables.

Regression equations are like functions but for the fact that the values for Y are some kind of averages, so that if we make explicit a particular numerical value, we are in fact only producing 'the best guess' (guessing the average gives the slightest amount of errors in the long run, both in total magnitude and in the percentage of wrong guesses).

It is easy to imagine what hinders criminologists from using regression

analysis to any great extent. First of all, we seldom know the relevant values of a population variable; secondly, if we knew them they would more often than not be of the *discontinuous* kind that are measured on nominal scales, like: recidivist – non-recidivist; future delinquent – future non-delinquent; burglar – thief; rapist – robber. For variables measured at the nominal level, hardly any quantitative technique is admissible.

Even if, by some arbitrary decisions, we could manage to transform an originally nominal scale into an ordinal one (like the Glueck's did when they attributed rank order numbers to the kind of supervision a child got from his parents) we would still be measuring at the ordinal level only, not yet at the so-called interval level (the level of constant, significant differences between variables) which is the minimum requirement for legitimate application of this technique.

Further, where most relationships among variables in behavioral science are of a higher order, coefficients become hard to calculate and still harder to interpret. Their interpretation implies the need of a theory.

Lastly, one should keep in mind that even if all necessary conditions could be met (continuous variables, measured at least on an interval scale) we would in practice never meet true functions in behavioral science but only relations, which means that every value of the independent variable(s) may be mapped into more than one value of the dependent variable which we call our criterion. For interpretation we should need a probabilistic theory.

What regression analysis produces in reality is only a kind of pseudo-function, in which what is considered to be the specific value of Y is, in fact, the mean of all values that have been observed. As has been said, by predicting this mean instead of some other value one minimizes of course the total *percentage* of wrong guesses, as well as the total amount of wrong guesses, if the Y-values are normally distributed (which means the coincidence of mean and mode). If the distribution is not normal, one might in extreme cases make a wrong guess every time one predicts the mean as the most probable outcome, yet the total value of deviations from the true value of Y would be minimized. In that case one would have a good strategy, but with little theoretical impact.

5. In criminological practice we might want to predict the criterion recidivist – non-recidivist for predictors that are themselves elements of some typology. For instance we might like to know the likelihood of recidivism

for, say: rapists, thieves, joy-riders, burglars, murderers, given a certain cluster of conditions that are held constant, like age, sex, socioeconomic background and such. In that case it is definitely inadmissible to use the common types of prediction techniques and we would have to rely on so-called non-metrical analysis because of the nominal level of measurement of our independent variables. (Sometimes researchers use techniques that they know to be inadmissible and unreliable, for the sole reasons that they do not master any adequate technique that is mathematically much harder, and because experience has shown that applying the 'wrong' technique works at least better than blind guesses. This may be wise strategy in an applied branch of criminology, but it hardly merits the name of scientific endeavour).

Criminologists like the Glueck's who – together with Mannheim and Wilkins – have made the most well-known contributions to criminological prediction, have tried to overcome this problem while evading the use of non-metrical techniques by applying a rather subjective ordinal scale to their observed predictors, for example by qualifying supervision of a child as bad – rather bad – good – very good, or the like.

This, however, does not solve the problem completely, since ordinarily prediction is meant only for data measured on an interval scale, much information is lost when an ordinal scale is used, and what is worse is that the ordinal scale, if at all present, is supposed to come into the picture only at the *criterion* end of the relationship, not along with the predictor variables. We could of course reverse the prediction process and predict X from Y, if that were feasible under circumstances, but if *both* scales are ordinal we are no better off technically.

What we would need in criminology, but which hardly exist even in the behavioral sciences that are further developed at the experimental – statistical level, are non-metrical analyses of the following type:

Given a data-matrix where the possible qualifications which each of the nominal values X can take are either 1 or 0, ideally we should be able to weight these factors one by one with regard to the criterion Y, eliminate overlap and then predict quantitatively with the help of these weighted factors. This is in essence what the Glueck's have sometimes tried to do, only without eliminating this overlap. For instance they have been treating employment of father, alcoholism of father and supervision by father as totally separate predictors.

6. The predictions of the Glueck's suffer from three major deficiencies. Techniques which are admissible only at the interval level and above, or at the ordinal level and above, are sometimes used in their criminological prediction at the nominal level without flinching.

Prediction on the basis of regression equations, for instance predicting for a member of a given criminal population, say penal institution A, what is likely to happen after his release (like the very good early Borstal-type predictions) is avoided and instead estimation techniques which extrapolate the findings for a sample to the population are used. This is what for example the Glueck's did when they constructed their social prediction table to discriminate between future delinquents and non-delinquents for any population of as yet non-delinquent small children.

The technical flaw in their reasoning due to their taking a 50–50 sample of delinquents and non-delinquents as sampling basis for their estimation, has been pointed out by several authors. If the real percentage of delinquents in a population lies around, say, 18, one will get a lesser percentage of error-guesses by following a random strategy than by applying the Glueck's prediction table. If one does not realize this, the relative success of the Glueck's prediction table will seem remarkable.

However, even if the Glueck's sampling had been correct there would still have been a fundamental methodological problem. A problem that would remain even if the efficiency of the prediction table were as excellent as the Gluecks' and their disciples claim.

Suppose that a physician is to estimate for the members of a population – on the basis of his observations of a sample – whether or not they will be ill in, say, the next five years. He might be able to do this very well or at least better than anybody else.

One would, however, still not know much, if the concept of illness encompassed such diverse entities as a broken leg, venereal disease, stomach ulcers, blindness, epilepsy, eczema and neurotic depressions. Nevertheless this is the case in criminology when predictions are made about people becoming delinquent in the future or not, becoming a recidivist or not.

As we have already indicated, criminal behavior is not just one class of behavior, and at least four or five distinct categories can be distinguished which are irreducible to each other. Whether one commits an act defined as criminal which is objectively observable (like e.g. burglary), or one which is only subjectively interpreted by the judiciary (like libel, blasphemy, in-

decent behavior or involvement with 'pornography') makes a big difference. Further, committing an act (defined as criminal) for reasons that are inevitable either because of some inner compulsion (e.g. compulsive exhibitionism) or of compulsion from outside the actor (e.g. some traffic criminality) also makes a big difference.

So-called criminality is not a product of the actor alone, but also of the intentions of legislators, police, prosecutors, judges and juries (where they exist). Reasoning as if criminality in general were really one category of behavior, predictable from factors concerning the criminal only, is reasoning wrongly, even if the predictions happened to be accurate. *Ex falso sequit omnia:* One might well infer rightly from the premiss that the USA and Europe are linked by a series of long bridges across the ocean, that there is much interaction between American and European culture. So from personality traits of delinquents their future being prosecuted and convicted.[10]

7. It is possible to apply mathematical techniques other than the current statistical one to prediction in criminology. Before we go into this, it is necessary to consider the role of statistics in the science of criminology. We might conceive statistics to be a theory of sorts in its own right, namely a theory based on a mathematical system, interpreted as a probabilistic model and tested in some way (empirically) in the past. In that case statistical predictions would be falsifiable like statements derived from any theory, and there would be reason for great satisfaction if some criminological prediction were confirmed under the confidence interval chosen. The methodological question then is: how was the statistical 'theory' arrived at? On what experimental evidence is it based, and how were its hypotheses tested in the experimental phase if not by statistical means?

On the other hand we could consider statistics to be a model like any mathematical one, and statistical predictions to be just like other model-based predictions. In that case the question arises: what is the practical use of these predictions? We know that in principle almost any internally consistent model will suffice to make predictions that are reasonably accurate. (Cf. the several cosmological models that all describe, explain and predict the empirical astronomical data rather nicely on quite different –if not outright false – assumptions).

Models are rejected only if things become more chaotic and unclear with them than without them. Models structure the empirical data, classify them

and suggest that there is sufficient isomorphism between the formal relationship among their logical elements and the relationship between the empirical data they are mapping.

If we agree to think of inferential statistics as just one probabilistic mathematical model (which has the advantage of greater simplicity and plausibility over thinking of it as an empirical theory of sorts), a few considerations should be kept in mind:

1. the statistical model, like any model, is not falsifiable, though rejectable;
2. some low-level theoretical propositions are involved,
   a) when one decides to what population an element belongs;
   b) when one decides which boundary conditions should be regarded when extrapolating;
   c) when one decides how to match sample and control-group in an experimental set-up. This is particularly important in criminology, where the subject matter is behavior defined as criminal.)

8. We might be tempted to look for models other than that of inferential statistics to help us explain and predict relationships between elements belonging to the criminological universe of discourse. It should be noted that at the experimental level of investigation statistics is the only model at our disposal, but any kind of investigation preceding or following the experimental stage (or the equivalent of it in behavioral research) has a wealth of other mathematical models to choose from.

These other models are hardly ever used for two reasons it seems to me. Due to a lack of mathematical training (at the still elementary level most chemists, biologists, economists and some psychologists and sociologists in the best equipped universities use to get), most criminologists are just not equipped and therefore not motivated to use them. One might ask professional applied mathematicians to carry out the necessary procedures, but even to be able to ask the right questions one needs just a little knowledge of mathematics.

Further it may be so that although the kind of predictions that the statistical model produces is in itself strictly neutral, it is apt to be confused in practice with the causal model that the older generation in particular adheres to (explicitly or implicitly) in applied criminology: the so-called 'etiological' school, conceiving crime as caused by a number of factors 'in' the criminal. (Whether these factors are supposed to be things like

'immorality', 'weak super-ego', 'psychopathy', or something else is irrelevant here).

To make full use of, for example, predictive mathematical models different from that of statistics, one would also have to rearrange one's (often implicit) causal thinking.

In traditional prediction studies and their application, although one knows or should know that predictor variables are chosen solely for their predictive power (if the type of ear-lobe were the best predictor we would use it), there is a tendency to gradually forget this and to think of certain predictors as causally related to crime. The Glueck's especially, when they intend to use their prediction tables in crime prevention, sometimes show this lapse of memory, though at other instances they deny the interpretation of predictors as causal factors.[11]

In general, the causal model is a rather dangerous one because of the three causal conditions that have to be met as discussed above, among which especially the third condition: *lack of spuriousness* is a hard one to establish. Most empirical sciences show a tendency to discard causal models altogether as soon as they get better developed. One is no longer interested in *why* something happens (often an indication of an 'essentialist' bent of mind), but in interrelationships between variables, without any causal interpretation.

In criminology the causal model is particularly dangerous from a material point of view too: the one-sided attention its adherents devote to the perpetrator of acts defined as criminal leads them away from the important sociology of penal law aspect.

9. The set of behavior defined as criminal might be exhaustively classified in the following way:

1. Behavior at the discretion of the actor for which objective empirical indicators are present. (E.g.: crossing the street within two meters from the corner of the street is defined as criminal).

2. Behavior at the discretion of the actor for which no objective empirical indicators are defined, so that conviction is entirely at the subjective discretion of the judges. (E.g.: writing and selling 'pornographic' books; insulting religious beliefs of others; blasphemy etc.).

3. Behavior not at the discretion of the actor for which objective empirical indicators are present. (E.g.: compulsive exhibitionism (indicated in

Figure 4 as 3a) or behavior which is unavoidable for external reasons (3b) like some traffic offenses).

4. Behavior not at the discretion of the actor for which no objective empirical indicators are defined. (E.g.: alleged libel during a state of lowered consciousness in an epileptic).

Figure 4

indicators	f	$\bar{f}$
obj.	1	3 a/b
subj.	2	4

f = 'free' act
$\bar{f}$ = 'unfree' act

The concept of 'free' respectively compulsory behavior is of course a difficult one that would merit a separate discussion. It all depends on one's discipline, say law, psychology or sociology, and the school of thought one adheres to within that discipline, how the definition of these concepts will be constructed. D. R. Cressey has published an article[12] in 1962 in which he tried to demonstrate that from a sociological point of view, more specifically from the point of view of so-called role-theory, the concept of compulsory crime is redundant. Either the criminal act so defined is 'motivated' just like any other act (learned through social interaction and afterwards defined by 'motivational' linguistic constructs that have been learned through social interaction as well) or it is an 'unmotivated' act of insanity and then, by definition, not an act of crime.

Cressey's attempt to get rid of any unobservable (hidden') motives and to concentrate instead on the (observable) motivation for (observable) acts has both its charms and its drawbacks it seems to me. Its charm is that in this way one can stick strictly to a sociological model of behavior; no psychological innerness is needed for the explanation of behavior. Its drawback is that the rigorously sociological model seems a bit artificial from a (present day) common sense point of view in which traditional psychological insights have been incorporated already, whereas strictly sociological concepts and models are still rather alien to it.

Anyway, apart from the necessary explicit definition of $\bar{f}$ and f acts, the

above classification may be considered as fundamental, over which other classifications, like the traditional judicial one or the sociological one constructed by Sellin and Wolfgang[13] could be distributed.

Since the behavior defined as criminal which belongs to cells 3 or 4 of the model is not at the discretion of the actor, it is not surprising that ordinary penal treatment shows no effects with regard to recidivism or perhaps change for the worse (Wilkins).[14]

The behavior which belongs to cell 1 is primarily dependent on criminal legislation (changing policy of defining and prosecuting criminal behavior).

Upon reflection it is not astonishing that traditional statistical prediction in applied criminology, which fails to make a distincttion between the several fundamental categories of what will be defined as criminal behavior, is a poor tool. We need a classification of behavior defined as criminal in a given state and at a given time in the four cells mentioned. Then for cell 1 and cell 3 if no effective treatment has been given, we could keep the legal defining, detecting and sentencing policy constant and continue to predict in the traditional way, that is predominantly with statistical estimation procedures. For cells 2 and 4, however, we would have to predict the subjective interpretation of judges. Also, in cell 3, we would have to distinguish between behavior that is unavoidable for reasons inherent in the actor as an individual (his psychological or biological condition) and behavior that is just as 'unfree', but for reasons that are linked with the structure of society. Among the latter may be both quite simple and sociologically complex situations. Two examples:

a) Imagine that there are X_1 legal parking places for cars, where social structure and norms make it necessary to park, say, X_2 times every 24 hours. If everything could be that simplified and accurately known, the perfect macro-prediction would be $X_2 - X_1$ criminal acts per 24 hours for this penal sector.

b) Complex situations leading to 'unavoidable' acts are there where subcultural norms clash with those of the dominant one. The chances for a lower-class boy in certain metropolitarian slum areas nòt to become a member of a delinquent gang are very slight indeed. The same goes for the *vendettas* of some ethnic groups, etcetera.

Among sociologists the necessity of conforming to subcultural norms, even if these may lead to (informal or formal) sanctions from the dominant group has been heavily accentuated. There exists much literature on this topic in relation to delinquency.[15] However, the link with prediction tech-

niques has not been laid, yet, as far as I know. As a qualitative point of view the 'theory' of subcultures will be discussed in the next chapter.

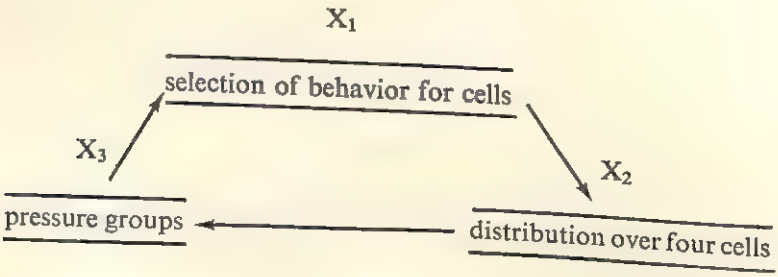
10. The problem of crime prediction (or crime causation if one wants to adhere to a causal model) is now reduced to the following:

If we could predict the distribution over the four cells of the (variable) set of behavior defined as criminal, we would know which methods of prediction to apply and how to combine them. (This condition cannot however be met as long as there is no sociology of penal law which studies on an empirical basis how the various pressure groups in society influence (the changes in) a penal system).

If sociology of penal law existed (in the sense of 'if it were developed as an empirical science') and if jurimetric methods (the compiling and analyzing of data with regard to penal law, jurisdiction and penal enforcement, plus the necessary simulation techniques, all of course to be done with the help of a computer) were fully developed the changes in (distribution of) behavior defined as criminal and the changes in penal policy could be studied with the help of a model for non-continuous, discrete functions, applying sets of difference equations.

One could start with a simple graph where pressure group, type of behavior defined as criminal and distribution of the four fundamental types of acts might be indicated as follows:

Figure 5



For the sake of simplicity we shall not only consider the selection as dependent on the pressure groups and the distribution as dependent on the selection (see Figure 5), but also the influence of pressure groups as dependent on the resulting penal system. This is of course an extremely simplified

model. Only a computer could do the work if we considered all the relevant variables influencing X_3.

Now if we indicate the selection of behavior defined as criminal at time t as X_{1t}, the distribution of this behavior over the four cells of figure 3 at time t as X_{2t}, and the power of the combined pressure groups at time t and at $t-1$ as X_{3t} and X_{3t-1}, we get:

$$X_{1t} = a_1 + b_{13}X_{3,\,t-1} + u_{1t} \tag{3.1}$$

$$X_{2t} = a_2 + b_{21}X_{1t} + u_{2t} \tag{3.2}$$

$$X_{3t} = a_3 + b_{32}X_{2t} + u_{3t} \tag{3.3}$$

where u is an error term allowing for neglected exogeneous variables.

We see from the equations that the selection of behavior defined as criminal at time t is dependent on the power of pressure groups at time t–1.

By substituting equation (2) into equation (3) we get:

$$\begin{aligned} X_{3t} &= a_3 + b_{32}(a_2 + b_{21}X_{1t} + u_{2t}) + u_{3t} \\ &= (a_3 + b_{32}a_2) + b_{32}b_{21}X_{1t} + (b_{32}u_{2t} + u_{3t}) \end{aligned} \tag{3.4}$$

By subsituting t–1 for t in equation (4) we get:

$$\begin{aligned} X_{3,t-1} &= (a_3 + b_{32}a_2) + b_{32}b_{21}X_{1,t-1} + \\ &+ (b_{32}u_{2,t-1} + u_{3,\,t-1}) \end{aligned} \tag{3.5}$$

And by substituting this equation into (1) we get;

$$\begin{aligned} X_{1t} &= a_1 + b_{13}(a_3 + b_{32}a_2) + b_{32}b_{21}X_{1,t-1} + \\ &+ (b_{32}u_{2,\,t-1} + u_{3,\,t-1}) + u_{1t} \\ &= (a_1b_{13}a_3 + b_{13}b_{32}a_2) + b_{13}b_{32}b_{21}X_{1,\,t-1} + (b_{13}b_{32}u_{2,\,t-1} \\ &+ b_{13}u_{3,\,t-1} + u_t) \end{aligned} \tag{3.6}$$

which can also be written as:

$$x_{1t} = A_1 + b_{13}b_{32}b_{21}X_{1,\,t-1} + (U_{1t} + U_{2,\,t-1} + U_{3,\,t-1}) \tag{3.6}$$

Thus, in equation (6) we have expressed X_{1t} (the selection of behavior defined as criminal) as a function of the selection at time t–1, while a combined error term for neglected variables has been added.

Graphically we have a *loop:* a change in the selection of behavior defined as criminal will eventually influence a future change in this selection (a similar procedure might have been applied to penal policy), that is, a difference equation has been produced, for which the coefficient of $X_{1,\,t-1}$ has to be interpreted (a rather complicated practical problem which will not be considered in this context).

Once selection of behavior defined as criminal and penal policy are described mathematically, we have a fundamental prediction model, to which can be added the boundary conditions (or individual 'causes' in tradional language) for which this selection works.

That is to say: for acts in cell 1 of our classification model equation (6) will suffice and the individual variables that have been neglected will be included in U_1. For acts in cell 3 (*a* and *b*) the external or internal 'unavoidability' will have to be accounted for either by U_1, or better still by additional mathematical procedures. For acts in cell 2 or 4 the variables that lead eventually to the (subjective) interpretation of a certain act as belonging or not to the negative category of a given penal norm, should be accounted for by additional mathematical procedures.

11. We have indicated how the selection of acts to be defined as criminal can be seen as a closed dynamic process (not necessarily in equilibrium), for which the traditional individual predictors can be considered as boundary conditions or can be viewed as a sub-set of the collective error term. However, selection of crime implies two processes: the definition of which deviant acts will be classified as criminal by those empowered to do so (legislators and the pressure groups behind them), and the classification of any given act as belonging to the category defined as criminal.

Where objective indicators for a criminal act exist (theft and burglary for instance) the formal procedure gives judges a wider latitude for subjective interpretations and arbitrary decisions than one would expect. However, although for example white-collar crimes for which objective indicators exist often are not prosecuted or convicted because of a different definition of the situation (e.g. drug trafficking and lethal negligence by doctors; fraud by very large buisiness concerns, homicide by members of the armed forces etc.), I shall not discuss this now, but concentrate on subjectively indicated crimes only, for which 'pornography' might serve as a good concrete example.

Deciding whether a deviant act should be defined as criminal, and deciding (e.g. in court) whether an act subjectively indictable only belongs to the set of behavior defined as criminal are of course two different processes. In the former instance the time-lag (discrete time) made us choose a difference equation model. In the latter case the decision to interpret subjectively, for example whether or not a certain publication should be

defined as belonging to the pornographic category, is probably best seen as an optimalization problem.

In addition to our fundamental classification model, it will also be necessary to assume that judges (or magistrates in general) are confronted with two kinds of suspects: those whom they may feel sympathetic to for any reason (similar socio-economic and cultural background for instance) and those whose subcultural norms or – in their eyes – deviant status make it hard for them to feel any empathy at all. This may influence their choice in the definition of acts that can only be subjectively interpreted (not objectively observed) as belonging to the class of criminal behavior.

Further, we must assume that they will not want to seem prejudiced, not in their own opinions nor in that of the public (this goes especially for a democratic society; in an extremist kind of state prejudice may be honored under certain circumstances), so that they will be motivated to introduce a random element into their interpretation of acts for which no measurable or objectively observable indicators are defined.

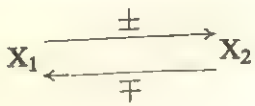
If all of these evidently time-continuous processes were linear, we would have three linear regression equations and the optimalization would be easy to predict. In reality, some or all three of these processes may be best described as differential equations, and their interpretation may be difficult or unachievable.

If magistrates showed no tendency to unbalance the predictability of their decisions by introducing a random element, we could easily predict that if X_1 = emotional sympathy (again invoked by subjectively interpreted indicators), X_2 = decision to convict or not (probably primarily dependent on the result of the first decision, though also on exogeneous variables), but X_1 among others dependent on earlier decisions about X_2 (once convicted one gets more easily convicted again) we have a simple, semi-stable feedbacksystem, where only the randomization, calculated into the error terms for X_2, and other exogeneous variables allow for dynamic processes.

(It should be noted again that we are considering here only those acts defined as criminal for which the indicators are only subjectively interpretable, so that X_2 is really at the subjective discretion of the judge).

This system can be illustrated graphically as follows:

Figure 6



If the decision for X_1 is positive, we will have a negative decision for X_2 and vice versa.

Algebraically we would have two equations:

$$\frac{d X_1}{dt} = b_{11}X_1 + b_{12}X_2 \tag{4.1}$$

$$\frac{d X_2}{dt} = b_{21}X_1 + b_{22}X_2 \tag{4.2}$$

where b_{11} and b_{22}, or generally b_{ii}, are the feedback terms and b_{12}, or generally b_{ij}, represent the influence of the variables on each other.

We have only a qualitative indication of the situation so far, which, however, might be substantiated by quantitative measurements.

Let us assume here that the optimal decision for judges will imply indeed some randomization. (It would be a task for jurimetric analysis to prove, specify or falsify this hypothesis). By differentiating with respect to t such a randomization effect has been lost from sight here. We could allow for a randomization effect by implying that feedback from X_1 to X_1 will eventually become negative and so will feedback from X_2 to X_2.

12. We have not yet discussed the prediction of those crimes that are, for all practical purposes, inevitable, either for reasons internal to the actor (e.g. compulsive acts) or for reasons external to him (the shortage of legal parking places and norm-conflicts have been given as examples). In the type of case illustrated by the example of parking shortage, from the viewpoint of macro-prediction, we are confronted with a mathematically extremely simple, empirically probably very hard to quantify, subtraction.

From the viewpoint of individual behavior, we should use a probability function. The estimation techniques used up to now in prediction in applied criminology are the right approach in that case. (We take the behavior of a delinquent known for this type of offense as our sample and draw inferences about his future behavior in this field 'all other things being equal', or implying the same boundary conditions).

If criminological prediction tables had been restricted to a single fundamental category of deviant behavior defined as criminal, their power to predict would not have been leveled out so much by other types of crime, for which individual predictors are little relevant.

Prediction would improve still more if we could combine the prediction equations for the different categories mathematically, that is if instead of

our disturbing error terms, we could allow for the other processes. For instance even in the case of unavoidable acts, there remains the changing definition of the situation and the changing penal policy of magistrates as a disturbing influence. The integration (not in the technical sense) of the four mathematical approaches should be our final aim.

13. When applying mathematics to empirical science one must of course remain constantly aware of the fact that it is, alas, impossible to pull oneself out of the water by one's own hair, that is: mathematics is a mere tautological system, and no amount of mathematical reasoning will lead to theoretical insights if the symbols are not substantiated by empirical, preferably statistically interpreted research.

On the other hand, if one was lucky in choosing a model and identifying its elements, there may be derived some consequences on purely mathematical grounds that would have remained obscure if only a verbal model were available. The mathematical symbolism used in the above (graph and equations) I adopted from a book by H.M. Blalock, Jr.[16] that has nothing to do with criminology (or sociology of deviant behavior, sociology of penal law and applied criminological prediction) whatsoever. It seemed to me that one could use them as a theoretical model and its main aim has been not the rough application of existing mathematical approaches to criminological situations, but to try and shift the attention of empirical researchers from (fruitless) individual prediction that is based on current ideologies concerning the criminal as an exceptional human being towards more critical (fundamental) research.

However, that mathematics may be used just as well from the traditional point of view in what I have called applied criminology, is proven by an article by M.E. Wolfgang and H.A. Smith.[17] Here the new tools were applied to the old individual-etiological insights.[18]

14. In describing explanation and prediction with regard to criminology, there remain to be discussed two more problems, namely that many explanations (and thus implicitly too: predictions) in behavioral science are probabilistic and/or are supposed to be of the functional or teleological kind.

Let us treat probabilistic explanation first. Hempel, *op cit.*, p. 59, makes them explicit in a way analogous to deductive-nomological explanations. Instead of deterministic laws we now have laws that are formulated in

probabilistic terms. His example: 'The probability for persons exposed to the measles to catch the disease is high.'

Then there are one or more particular facts, just as in the other type of theoretical explanation. In the example given by Hempel: 'Jim was exposed to the measles.' Then follows the explanandum: 'Jim caught the measles'. but separated from the explanans sentences by a double line instead of a single one, indicating: 'makes highly probable'.

Schematically:

Explanans	$L_1 \ldots L_n$	(probabilistic laws)
	$C_1 \ldots C_n$	(statements of antecedent conditions)
Explanandum	E	(makes highly probable)

In Hempel's own words: 'In both cases, the given event is explained by reference to others, with which the explanandum event is connected by laws. But in one case the laws are of universal form; in the other of probabilistic form. And while a deductive explanation shows that, on the information contained in the explanans, the explanandum was to be expected with "deductive certainty", an inductive explanation shows only that, on the information contained in the explanans, the explanandum was to be expected with high probability,'

So what remains to be considered, now that the analogous aspects of both types of explanation have been stressed, is what differentiates the latter from the former, that is: the nature of probabilistic laws and the nature of probabilistic implication.

In the first place one should be aware of the fact that probabilistic explanation concerns a relation between *sentences* (linguistic structures), not between events. This means that, as has been defined in the foregoing chapter (Ch. III, 6), we are concerned in this case with *logical probability* ('a value p is given to the validity of the inference itself'), not with statistical probability (when a value p is given to the relative frequency of an event), and with probabilistic *statements* about laws.

What we are doing in the case of probabilistic explanation, is making an inductive subsumption under laws of probabilistic form (instead of making a deductive subsumption under laws of universal form). Hempel, *op. cit.*, p. 69, rightly remarks that in physical texts theoretical accounts of this probabilistic kind are generally referred to (accepted as) explanations. So

there is no reason for behavioral sciences to feel inferior or peculiar if they have to rely heavily on this type.[19]

15. The matter of teleological (or functional) explanation (and prediction) is again one that would require a volume for itself.

Again we should make a sharp distinction between teleological statements and teleological systems. But contrary to when we discussed probability, we cannot quite neglect here the ontological level. Whether or not the world is deterministic (or partly so) at the ontological level is a matter of little importance in everyday behavioral research. We use probabilistic models when validating or theoretically interpreting our hypotheses, but in the discovery phase of our science we are hardly concerned with it (unless we would use abstract mathematical models of probabilistic form for that purpose). That behavioral science is concerned with teleological (purpose oriented) systems is another matter. One cannot neglect this difference between our subject matter and that of for example physics without becoming highly artificial in one's constructs.

Rudner, *op. cit.*, pp. 84–111, analyzes a set of teleological statements. Some of them, like 'John works hard with the purpose of passing his examination next month' are only teleological in form. In reality, John's behavior is motivated not by the future examination, but by his present wish to pass it. Others, which one may find in (older) biology and medicine texts ('The fur of the polar bear is white in order that he may not be easily detected by his prey') have long been learned to reformulate by fervent evolutionists. ('Bears who happened, by accidental mutation of genes, to have a white fur, had the best chance of survival. So natural selection among species made them dominant in polar regions.') With this type of teleological statements we do not need to be puzzled by future 'causes', if only we succeed in reformulating them, which is easy. In the words of Rudner, *op. cit.*, p. 87:

'With the contributions of Charles Darwin, nonseparatist analyses of these statements have become routine. In taking the statement explanatory, the nonseparatist will construe it to be severely elliptical. He offers an alternative that (a) is scientifically or technically equivalent to the original ellipsis and (b) involves a methodology of explanation not different from that appropriate to nonteleological phenomena.'

This solves the formal problems, yet behavioral scientists are aware of the fact that in many instances their subject matter consists of teleological

systems and/or teleological entities and most of the time teleological systems that are composed of teleological entities.

To be able to describe teleological systems we need first of all a definition of their 'opposites': deterministic systems.

Def. A system is deterministic with regard to certain variables, then and only then if there exists a deterministic theory with regard to these variables.

This means that given the description of the state of the system at time t, we can make deterministic predictions about its state at time $t + 1$, or deterministic postdictions (or retrodictions or explanations) about its state at time $t - 1$.

Now the difference with a teleological system is, that a teleological system has preferred states. One may have to refer to predictions of a probabilistic form to predict future states or postdict antecedent states. But now we can refer to our discussion of probabilistic explanation in the foregoing paragraph. Although there is a difference between deterministic and probabilistic explanations (or predictions), the latter are found in physical sciences as well. This means that if one has to take into account the teleological structure of social systems, one will often need explanations and predictions of a probabilistic form. But this does not reinforce the 'separatists' argument that because of the mostly teleological subject matter of behavioral science ordinary validation and theoretical interpretation would be impossible or useless.

We may grant the 'separatists' that theoretical interpretation is very hard indeed in our sciences, but not for the reasons they give.

16. Functional systems and functional explanations may be regarded as a subset of teological ones in general.

Def. An entity is functional for a teleological system if it is necessary for the survival of the system.

Instead of survival one may read, if social systems are concerned, the optimal output, or balance or efficiency. But al these concepts seem to be connected with survival in the long run.

Most so-called functional explanations in cultural anthropology and sociology are no explanations in the strict sense, but in fact descriptions of the system under consideration, or descriptions of the system plus what the scientist thinks it is 'really like' if manifest and latent functions (an otherwise useful dichotomy) are distinguished.

Real functional explanation may be achievable in principle but is much harder to come by than its defenders seem to think. Rudner, *op. cit.*, p. 108–109, gives a list of ten conditions that should be met if a functional explanation were really offered. I quote (p. 109): 'It is patent that nothing like the fulfillment of this staggering task has been accomplished in many (if any) of the works in which, to date, social scientists purport to be giving functional explanations. To give a functional explanation of the occurrence of x (more precisely to give a functional explanation of the occurrence of some characteristic of x) requires both that we explain the occurrence of x and that we provide an analysis of how x functions in a functional system' ...

'The results produced to date must be seen to amount only (so far as explanation is concerned) to the articulation of some prescientific hunches or pious hopes that a functional explanation for the item in question *can* ultimately be given.'

Rudner rightly adds that it is never substantial ignorance that one could reproach a scientist but only methodological ignorance. Only those functionalists who claim to have accomplished their task would be reproachable.[20]

NOTES

1. C.G. Hempel, *Philosophy of Natural Science*, Englewood Cliffs, N.J. pp. 49–54, analyzes deductive-nomological explanation.
See also: C.G. Hempel and P. Oppenheim, 'Studies in the Logic of Explanation', in: B.A. Brody (ed.), *Readings in the Philosophy of Science*, Englewood Cliffs, N.J. 1970 (pp. 8–28). The latter is the reprint of an article originally published in 1948. There is much fundamental methodological literature on explanation (and prediction), for which one might consult the Bibliography added to the latter article. However, from a criminological point of view a more extensive discussion would become irrelevant. I want only to mention in passing that in recent years there have been methodologists questioning Hempel's conception of explanation and his proof for the identity (in a logical sense) of explanation and prediction. This criticism is mentioned in A. Kaplan, *The Conduct of Inquiry: Methodology for Behavioral Science*, San Francisco, Calif. 1964, (pp. 346–351).
For the arguments of an authoritative critic see: I. Scheffler, 'Explanation, Prediction and Abstraction', in: A. Danto and S. Morgenbesser (eds.), *Philosophy of Science*, Cleveland, Ohio 1967 (pp. 274–287).

Another opponent is M. Scriven. See his article 'Explanation, prediction and Laws' (originally published in 1962) in: B.A. Brody (ed.), *op. cit.* (pp. 88–104). Scriven tries to prove his point by the example of explanation of an accident: the stain produced by the knocking over of an ink bottle. One could very well 'explain' this fact without knowing any laws of physics, he says. One must agree, but it should be noted that what Scriven has in mind is no explanation of a theoretical kind at all. We might better denote it by *explanation*$_2$ as contrasted to (theoretical) *explanation*$_1$. What Scriven is really writing about is either psychological elucidation by a reduction to the familiar, or by stressing the causal chain of the events. He writes: 'You knocked the ink bottle over. This is the explanation of the state of affairs in question, and there is no nonsense about it being in doubt because you cannot quote the laws that are involved, Newton's and all the others.' (In: Brody, *op. cit.*, p. 90).

It is evident from this quotation that Hempel and Oppenheim *cum suis* have in mind something quite different from what the opponents may be thinking about. Yet even the fact that they are thinking of evaluation at different levels of abstraction is in itself significant. G. Sjoberg and R. Nett in their book *A Methodology for Social Research*, New York 1968, rightly remark (p. 289) that the real discussion seems to be between (neo-) positivists (like Hempel) and the *Verstehen* adepts.

Typical for the latter is exactly their halting in the discovery phase of scientific activity (connected with psychological *explanation*$_2$) and their neglect of empirical tests (validation) and theoretical interpretation based thereon.

2. R.S. Rudner, *Philosophy of Social Science*, Englewood Cliffs, N.J. 1966, p. 60.

3. C.G. Hempel, *op. cit.*, p.60.

4. D.R. Cressey and D.A. Ward, (eds.) *Delinquency, Crime and Social Process*, New York 1969.

5. For diferential association theory see E.H. Sutherland and D.R. Cressey, *Principles of Criminology*, Philadelphia 1966[7] (pp. 77–83).

For anomie theory see R.K. Merton, *Social Theory and Social Structure*, New York 1968 (1957), pp. 175–248.

For interactionism see H.S. Becker, *Deviance and the Response of Others*, in: *ibidem*, *Outsiders*, New York 1966, pp. 8–14. Also: F. Tannenbaum, *Crime and the Community*, Boston 1938; E.M. Lemert, *Social Pathology*, New York 1951; J. I. Kitsuse, 'Societal Reactions to Deviance: Problems of Theory and Method', *Social Problems*, 9 (Winter, 1962), pp. 247–256.

See also E. Rubington and M. S. Weinberg, (eds.)., *Deviance: The Interactionist Perspective*, New York 1968.

For the theory of subcultures see A.K. Cohen, *Deviance and Control*, Englewood Cliffs, N.J. 1966, pp. 65–66, 84–88, 93–97, 107–109. Further his *Delinquent Boys:*

The Culture of the Gang, New York 1955, pp. 49–72 and his article 'The Sociology of the Deviant Act: Anomie Theory and Beyond', *Am. Sociol Rev.*, 30 (Febr., 1965), pp. 5–14.
Also: R.A. Cloward and L.E. Ohlin, *Delinquency and Opportunity: A Theory of Delinquent Gangs*, New York 1960, pp. 124–143.
W.B. Miller, 'Lower Class Culture as a Generating Milieu of Gang Delinquency', *Journ. of Social Issues*, 1958, No. 14, pp. 5–19.
J. F. Short, Jr. and F.L. Strodtbeck, *Values and Gang Delinquency: A Study of Street-Corner Groups*, Chicago 1965, pp. 47–76.
For a general overview: L.T. Empey, 'Delinquent Subcultures: Theory and Recent Research', in: Cressey and Ward, *op. cit.*, pp. 709–712.
For labelling theory see S. Wheeler and L.S. Cottrell, Jr., *Juvenile Delinquency: Its Prevention and Control*, New York. 1966, pp. 22–27.
For the scapegoat version of labelling theory: D.H. Chapman, *Sociology and the Stereotype of the Criminal*, London 1968.
For secondary deviation: E.M. Lemert, *Social Pathology*, New York 1951, pp. 70–71, 75–78.
The respective assumptions of these several 'theories', their differences and their overlap will be treated in the next chapter.

6. H. Hyman, *Survey Design and Analysis*, Glencoe, Ill, 1955, Chs. 5–7.
T. Hirschi and H.C. Selvin, 'False Criteria of Causality in Delinquency', *Social Problems* (Winter, 1966), 13, pp. 254–268. Reprinted in: M.E. Wolfgang, et al., (eds.), *The Sociology of Crime and Delinquency*, New York 1970, pp. 127–140, also in T. Hirschi and H.C. Selvin, *Delinquency Research: an Appraisal of Analytic Methods*, New York 1967, pp. 114–141.

7. 'It is doubtful that in any other area of study would anybody think it worthwile to try establishing a school of thought identified only by the fact that a particular group of statistical or quasi-statistical techniques had been applied to a problem within the field concerned.' L.T. Wilkins in his *Evaluation of Penal Measures*, New York 1969, p. 60.

8. E.W. Burgess, 'Factors Determining Success or Failure on Parole', in: A.A. Bruce, et al., (eds.), *The Workings of the Indeterminate Sentence Law of Illinois*, Springfield, Ill. 1928.
One could retrace attempts at prediction to a still earlier date, as W.H. Nagel does in his complete overview up to 1965 *Het Voorspellen van Krimineel Gedrag*, The Hague 1965 (revised and enlarged English edition by W.H. Nagel and C.I. Dessaur under preparation). Nagel mentions 13 prediction efforts in criminology prior to Burgess (from 1888 onward) but he admits that the people involved should be called forerunners only and that Burgess constructed the first real score table. Wilkins *op. cit.*, p. 65, is of the same opinion.

9. Wilkins, *op. cit.*, pp. 61–62.

10. The truth table for material implication is:

P	q	$P \rightarrow q$
T	T	T
T	F	F
F	T	T
F	F	T

T = true
F = false
$\rightarrow$ or $\supset$ = 'implies'

11. The Glueck's deny that their social predictors are causes when they stress the importance of psychological variables (whether as concomitant independent variables or as intervening variables does not become quite clear).
'A factor, whether personal or *situational* does not become a cause unless and until it first becomes a motive.' (Sh. and E. Glueck, *Family Environment and Delinquency*, Boston 1962, p. 153; italics mine, C.I.D.). This implies that according to their reasoning (which, by the way, is logically incorrect: for their misinterpretation of the concept of cause see Hirschi and Selvin, *op. cit.*) the social factors of their social prediction table are no causes but relatively efficient predictors so long as the determining psychological factors are not known.
This point had been stressed by them already in their *Predicting Delinquency and Crime*, Cambridge, Mass. 1959, p. 265. But when Sh. Glueck defends this point against the critique of A.J. Reiss, Jr. ('Critique of The Glueck Social Prediction Table', in: N. Johnston, et al., (eds.), *The Sociology of Punishment and Correction*, New York 1967, pp. 273–274; adaptation of the original article from 1951), and others, he writes: 'The choice presented to a community is whether its citizens prefer to let potentially delinquent children ripen into persistent offenders or to intervene' ... 'by aiding parents to modify their damaging disciplinary and non-affectional attitudes and practices' ...
(Sh. Glueck, 'Defense of the Glueck Social Prediction Table', in: Johnston, et al., (eds.), *op. cit.*, pp.275–281; adaptation of the original article from 1960).

12. D.R. Cressey, 'Role Theory, Differential Association and Compulsive Crimes, in: A. M. Rose (ed.), *Human Behavior and Social Processes*, Boston 1962, pp. 447–467.

13. Th. Sellin, and M.E. Wolfgang, *The Measurement of Delinquency*, New York 1964.

14. Wilkins, *op. cit.*, p. 76: 'Perhaps on the evidence supplied by Bailey we might conclude that most or all correctional treatments are increasing rather than decreasing the probability of recidivism.' ... 'By doing as little as possible we may be doing as little harm as possible.' (The research data on which this opinion is founded are to be consulted in: W.C. Bailey, 'Correctional Treatment: An Analysis of One Hundred Correctional Outcome Studies', *J. of Criminal Law, Criminol. and Police Sc.*, 57, 2, 1966, pp. 153–160).

15. See note 5 and the next chapter.

16. H. M.Blalock, Jr., *Theory Construction: From Verbal to Mathematical Formulations*, Englewood Cliffs, N.J. 1969, pp. 79–81.

17. M.E. Wolfgang and H.A. Smith, *Mathematical Methods in Criminology*, in: *Int. Soc. Sci. J.*, XVIII, 2, 1966, pp. 200–223.

18. Parts of this chapter until this point are an adaptation of C.I. Dessaur. *Prediction in Criminology*, Leiden 1970. (Paper presented at the 7th World Congress of Sociology, Varna, 1970. Working Group: *Mathematical Models and Methods in Social Prediction.*)

19. For our criminological purposes it would lead us much too far to discuss the controversies with regard to probability and induction. The subjects are treated extensively in A. Pap, *An Introduction to the Philosphy of Science*, New York 1962, pp. 139–247. In Chapter 9 and 10 of this book (pp. 139–173) the classical ideas about 'inductive sciences' are rejected (no science is totally inductive) and a classification of inductive sentences is given. Mill's and Keynes' theories of induction are discussed, Keynes' theory is preferred over the theory of Mill, but both are rejected because of the ontological fallacy.
Chapters 11–13 treat the frequency and the range interpretation of probability, the frequency interpretation (as elaborated by H. Reichenbach and R. von Mises) being analogous to what we have called 'logical probability' (the truth-frequency of inferential statements is stressed), the range interpretation being associated with the properties of a normal distribution of events. However, it is possible to treat both aspects of probability with the range interpretation, rejecting the frequency one, and this is what Pap does.

20. For a technical discussion of teleological systems, functional systems and the explanation statements they require see also R.B. Braithwaite, *Scientific Explanation*, New York 1953, pp. 319–341.

CHAPTER V

Some current types of criminological explanation

1. In the foregoing chapters, especially Chapter II on classifications, typologies and models, and further in the last chapter, where explanation and prediction in general were discussed, we have already met, be it superficially, a host of (alleged) theories and explanations in criminology. It will be evident from the text up to this point that criminology today lacks almost completely real theories; real theoretical explanations and predictions. There is no reason whatsoever for reproach or disappointment, if only authors, researchers and students in the field of criminology are aware of the methodological level they are working on and its limitations.

In the present chapter some current criminological 'theories' or types of 'explanation' will be elaborated, regardless of their respective progress in the direction of true theory and true explanation. I will in general omit parentheses around these terms from now on and try to indicate what the different (but also sometimes overlapping) schools of thought are like.

Let us first quickly dismiss some of the worst types of (happily already rather obsolete) typological explanation that is still around.

In Chapter II the typology of M. B. Clinard and R. Quinney as exposed in their *Criminal Behavior Systems: a Typology* (New York 1967) has already been criticized as well as the meta-typology that Th. N. Ferdinand gave in his *Typologies of Delinquency: a Critical Analysis.* (New York 1966). The same objections as I made against Clinard and Quinney's typology go for M. R. Haskell and L. Yablonsky's typology in their book *Crime and Delinquency*, Chicago 1970. Part Two of their book, containing Chapters 5–12 (pp. 75–222), titled 'Patterns of Criminality' makes the following distinctions:

Socialized Criminals
The Sociopathic Offender
Patterns of Violence
Organized Crime
Crime and Addiction
Psychedelic Drug Use and Abuse
The Sex Offender
White-Collar Crime

This overlapping (e.g. socialized – white collar; sociopathic – addicted – violent – white collar) and non-exhaustive (e.g. political criminals) typology is most dangerous, not only for its logical and methodological defects but because it really hampers the growth of theoretical awareness if students are educated with this kind of traditional, establishment-bound labeling. The typology suggests distinctions that need not be there (what to say about captains of industry that deliberately soil our environment for the sake of short-run financial gain or that pay lower wages to women and members of ethnic minorities, regardless of the local law and the Treaty of Rome, or that exploit, directly or indirectly, people from the developing countries, or that, directly or indirectly, stimulate war? Are they not violent, sociopathic, maybe addicted and almost certainly organized?).

From several points of view: logically, methodologically, substantially, theoretically, ethically, this approach to criminology and attempt at criminological explanation cannot be taken seriously.

The same cannot be said about so-called biological criminology (criminologists making a reduction to biological variables for explanation). In Europe the combination of biology (mainly genetics, physiology and, recently, ethology) and criminality is associated since World War II with fascism/national-socialism. It is hard to do research in this field and those who persist (mainly in Western Germany) are for the greater part rather suspect as to their political affinities and aims. (The criminological society in Western Germany has split on behalf of this point). Yet there are a few things in biological criminology that merit our attention.

Apart from the commitment of criminal acts due to biological pathology (which ought to lead to therapy instead of sanctions) there is the interesting question of the association between somatotype and delinquency.[1] It has been argued that even if such an association existed (e.g. between number of X- and/or Y-chromosomes and delinquency), for it to be useful as a (causal) explanation, too many intervening variables would be necessary.

This argument relies on a false criterion of causality, however, which will be discussed later on.

Further there is the highly interesting ethological point of view in regard to urbanism and aggression. Association between these two variables is supposed to be dependent on overcrowding and the neglect of deeply rooted territorium instincts (by the authorities that maintain certain types of housing and city plans).

The literature on this topic is as yet scarce and spread out over many publications. J. J. M. van Dijk wrote a (still unpublished) thesis in Dutch on it for his Master in Law degree at Leiden University.[1a] From this thesis I gathered that in the future we may expect some interesting explanations especially of aggressive criminality, including war, from ethological quarters.

But let us return to the current sociological, or sometimes social-psychological, criminological explanations.

2. Criminology today, even in its most enlightened form, is mainly concentrated on the sociology of deviant behavior (held punishable), neglecting the aspect of sociology of penal law and its interaction with this behavior. This is not entirely due to lack of insight or interest but certainly also to the very primitive state of sociology of law in general and sociology of penal law in particular.[2]

In general deviant behavior is explained at three basic levels of explanation: the psychological, the social psychological and the strictly sociological one.

At the psychological level kinds of people are distinguished, their respective drives or motives or compulsions are taken as independent variables and their behavior (deviant or non-deviant) is taken as the dependent variable. If some influence is granted to developmental background and experiences and to present situational variables, one gets an eclectic type of theory, or, if the situational variables get equal weight, a social psychological one.

The research strategy at this theoretical level (with the accent on the actor) is to devise a classification or typology of 'kinds of people', draw samples of the assumed types, take matched control groups and then count frequencies of deviant behavior in both experimental and control group and calculate whether the differences are statistically significant (and in the desired direction).

Schema (from A.K. Cohen)[3]:

Kinds of people	Behavior	
	Deviant	Non-deviant
P_1		
P_2		

A word of warning is apt here. Statistical significance is tested with chi-square, the T-test or other measures of significance. What does it mean, however, if a relationship between two variables is statistically significant? It means that it is indicated (by a number between 0 and 1) how great the chance is that the established relationship is due to chance factors.

If one takes a great number of psychological variables and/or a great number of types of deviant behavior the chance that the relationship between some of them will be significant is very high. To quote L.T. Wilkins[4]: 'The greater the number of independent tests on any sample, the greater the probability that some will be *significant* by chance.'

This implies that although theoretical interpretation ought to come chronologically after the testing of hypotheses, if these hypotheses are derived from a theoretical model that is 'poor' (according to metascientific or prescientific insights or according to its *power*, that is: the range of events it can account for) one will not get much from it.

Those criminologists that adhere to the model described above will mainly derive their inspiration from psychodynamic models in one version or the other or from the frustration – aggression theory that is also a kind of neo-Freudianism.

Some of the main positions among psychodynamic psychologists (all drawing heavily on Freud's mechanicistic instinct model or instinctual reductionism) have been analyzed by D. Feldman.[5]

According to Freud there are three basic psychological processes operating within the individual:

a) the original instinctual impulses or drives (of an essentially a-social or anti-social character), designated by him as 'the' ES. (The fact that his terminology has certainly led to hypostatizing this 'entity' which has originally been conceived as a collective term for certain dynamic processes is not essential here).

b) the mechanisms of adjustment, of coming to terms with reality: the so-called EGO drives.

c) the internalizing of group norms, mainly via parents and other educators, leading to cultural adaptation and a social conscience: the SUPER-EGO.

Now our psychological health is, according to Freud, dependent on the delicate balance between these three fundamental dynamic processes. If the balance is disturbed, in one way or another, one gets deviant behavior of one type or another. But not only is deviant behavior a product of disturbed balance between the three processes, it may also be an (inadequate) means of trying to restore the balance. (This latter point has been heavily stressed by neo-Freudians like K. Horney.)[6]

3. In respect to crime as a form of deviant behavior Feldman distinguishes five main positions in (neo-)Freudian explanation.

1. Criminality is a form of neurosis. Instead of ordinary neurotic symptoms the 'symptom' of criminal behavior is developed. The criminal neurotic is compulsively seeking punishment for unconscious (and poorly sublimated) incestuous wishes, that is his wish to have sexual intercourse with his mother and (in order to achieve this) kill his father.[7] (For females the reverse may be supposed to hold true, but Freud's mythology on females has always been lagging behind).

Criticism: The basic Oidipal assumptions are untestable and so are the consequences derived from them. Even if, for the sake of the argument, we would give Freud's rather Victorian and masculinistic verbal model the benefit of the doubt, it would still be evident that it is far from true that all criminals are seeking punishment. In his Victorian, *petit-bourgeois* model Freud certainly overlooked white-collar criminals, political criminals (for idealistic, humanitarian reasons), people who violate traffic or taxes laws, progressive social rebels.

The implicit assumption must be that 'real' criminals are the ones compulsively seeking punishment (but that means a circular argument!) or that conservative political behavior (regardless of the type of political state one is living in and regardless of the penal law it happens to have) is sane, resistance or rebellion against it neurotic.

Last not least: it has been empirically demonstrated that many criminals are far from neurotic.[8]

2. The criminal is an anti-social person who has been defectively socialized: his EGO drives are too weak with respect to his ES drives. In this case the delinquent does not look for punishment at all, on the contrary, if he

happens to get caught and punished, he will be unable of remorse but only feel hatred against the suppressors of his anti-social needs for gratification.

The criminal is a person who cannot delay satisfactions. The strong instinctual needs must be satisfied, at once.[9]

Criticism: Again the basic assumptions are untestable. Further: many criminals are perfectly able to delay satisfactions, e.g. white-collar criminals, organized professional criminals of the Maffia-type (among them are many manager-types, if not of the respectable kind), political rebels and so on. If one adheres to this type of explanation as the sole one for all criminality, one will again have to assume that the 'real' criminals are those that act as described, which means circular reasoning again. But even among the traditional criminals as conceived by the conservative bourgeois model of most Freudians there are many who are extremely well able to assess reality: e.g. great (professional) burglars, robbers, swindlers.

3. Criminality is a means of obtaining substitutive and compensatory gratifications. Everyone is striving for security, respect, success, status, self-expression, acceptance and such. If one's situation happens to be such that the respectable ways of achieving this are blocked, criminality may be a (disapproved) way of obtaining things or satisfying needs which would otherwise remain absent and unfulfilled, thus frustrating the individual unbearably. (We are here quite near Merton's anomie theory, as one will notice.)[10]

Criticism: None, except the lack of explanation for the fact that not all people thus deprived resort to criminal behavior. Indeed, most people are deprived in one way or the other but do not become criminals. Combined with a sociological learning theory this model may lead to satisfactory explanations, however, especially since it does not stress the criminal as an extraordinary being but, on the contrary, as an individual with quite ordinary needs but living in exceptional, frustrating circumstances. To make this type of explanation completely satisfactory one has to consider the very important (differentiating) social variables. Then this is already an eclectic standpoint.

4. The criminal is an anti-social character not because of a weak EGO but, because of a weak SUPER EGO. The SUPER EGO lacunae are due to the parents, who unconsciously were permissive towards deviant (criminal) behavior. The child, unconsciously noticing this unconscious permissiveness, acts as a good child should: according to his parents wishes.

Criticism: The unconscious internalization of unconsciously transmitted

defective social norms is a highly artificial, in any case untestable model. We know that there are children learning deviant (subcultural) norms from their parents, peers and other educators, but there is no need to regard this process as unconscious even if the relevant norms are not *verbalized.*

Again this model does not account for those types of criminality that have nothing to do with the point it stresses: the defective norms. It would be absurd if not outright immoral to define political criminals in fascist and communist dictator states, rebelling against terrorism and unjustice, as having unconsciously internalized unconscious defective norms of their parents.

If this standpoint was exclusively adhered to, it would mean an untolerable conservative ideology instead of scientific explanation.

Also people who get caught in a role conflict (say unemployed fathers or abandoned mothers without money who feel responsible for their children and may start stealing or frauding) can hardly be said to have SUPER EGO lacunae.

Like so many Freudian versions this is not science but conservative ideology and immorality, couched in pseudo-scientific terms. If we all believed in it, no minority would ever get emancipated, no suppressed group ever get freed.[12]

5. Criminal behavior occurs in social situations in which there is a severe discrepancy between dominant cultural norms (like becoming rich; successful) and the legitimate means of obtaining the institutionalized goals. The goals are all pervasive; the legitimate means are scarce. This is exactly like anomie-theory except for the accent the psychodynamic psychologists put on *reaction-formation* (overcompensation through overconforming) as a psychological process leading to the inevitable excessively individualistic and aggressive (anti-)social behavior.[13]

Criticism: According to this school of thought the decisive factor in becoming a criminal is one's inborn tendency to overcompensate. The frustrating social situation is there for everyone but the tendency towards reaction-formation is only found in certain people, namely those who become deviant or more specifically: criminal. The dichotomy between 'normal' people and criminals on psychological grounds is, however, untenable, as has been empirically demonstrated[14]. It might be, however, a limited explanation for a very limited group of criminals.

Yet, even if we allow for the fact that only some people are inclined to reaction formation, there are still those among them that do this in a non-

criminal way (the majority, finding outlets in football, war, tyrannizing their families and such). In the end we would need again social variables to explain the different solutions people choose who feel driven to over-compensation.

The conclusion so far is, that psychodynamic verbal models (or 'theories' or 'explanations') have little *power* (that is: a poor range of application), that many of their basic implications are essentially unfalsifiable (and thus fall outside the realm of empirical science), but are also vaguely and ambiguously defined, ideologically impregnated and far from logically consistent, thus falling outside the realm of axiomatic science or that of useful verbal models.

I am aware of two things:

1. If one is only intelligent and creative enough, almost any model will do to explain almost anything. The question is only: how efficiently does it do so, with what *power* and how fruitful is it for future research?

(There are religious or mythological models in use in primitive societies that explain everything satisfactorily from a psychological elucidation point of view. This does not lead to cumulative scientific insights however). Psychodynamic models seem very unsatisfactory for a future development of criminology as a fundamental science for the reasons mentioned under the criticism above.

2. When attacking Freudianism one is always fighting in a mirror-hall. Pious disciples of his verbal model will not reply in scientific terms but by explaining that those who feel the need to attack their leader, suffer from unresolved Oidipal complexes (e.g. hatred of authority) or such. (Thus taking what is to be proved as a basis for their explanation). Or they will say that one cannot understand Freudianism unless one has become an initiate oneself (through psychoanalysis in one of its versions).

I have tried to indicate what scientific grounds there are to reject the intrusion of most psychodynamic models in criminology as a fundamental science. In applied criminology, treating adjudicated criminals from any of these points of view may have some effect, regardless of their scientific soundness. Or, as the saying goes under medical men: 'Thrice a day a spoonful of water is still the best medicine; provided the doctor absolutely believes in it.' Or, as Clinard, *op. cit.*, p. 188, footnote 52, puts it: 'Many psychiatrists and psychoanalysts maintain that successes in therapy are proof of the validity of their theoretical systems. This is no more proof than

the "cures" of patent medicine. Other factors, such as the subject's belief and acceptance of the interpretation, as well as his personal relations with the analyst, also enter into the so-called successful treatment.'

4. Psychodynamic explanations of criminal behavior, strictly psychological ones, those that see a conjunction between the actor's personality and the social situation, all have in common that their primary interest is in the actor as an individual, their main explanatory variables individualistic ones.

This reduction to the individual has found its way not only in (for the greater part empirically disproved) criminological explanations but also in sociology itself. Structural-functionalism has taken over practically intact the Freudian schema of socialization, says Feldman, *op. cit.*, footnote 6. Merton's well-known 'manifest and latent functions' have clearly been inspired by psychoanalytic views on the unconscious (or subconscious) strivings of individual men. The concept of 'internalization' is not far removed from Freud's superego processes and we have noticed a close analogy between the explanation Healy and Bronner gave in 1936 for delinquency (see note 10) and anomie theory.

Quite opposite to traditional psychodynamic explanations of crime are those that look for relevant independent variables at the sociological level. The rate and distribution of criminality is a property not of individuals but of the social system. So independent variables are sought at the same level, namely in other properties of the system. The first to do so with great success was of course E. Durkheim[15] who proved that the rate and distribution of suicide was not a matter of individual motives but was dependent on certain properties of the social system. Individual motives were no more than intervening variables for the influences from the social structure and cultural norms upon a typical and rather stable suicide pattern per type of community.

Before Durkheim it was generally supposed that only one's personality made one take resource to suicide or not, just as in the last century many upper-class people thought that being unemployed (of others) was not due to the socioeconomic system but to the laziness or otherwise poor moral character of the unemployed person.

Among present-day strictly sociological explanations for criminal be-

havior one can distinguish some major (though partly overlapping) schools of thought:

1. *Cultural transmission theories* (partly including anomie-theorie, in so far as it does not accentuate the individual motives but the cultural process of transmitting dominant norms)

2. *Role-self theories* (including the concepts of reference group and of role-conflict) both often combined, to a greater or lesser degree, with:

3. *Interaction process concepts.*

Among cultural transmission theories we can distinguish:

1a. *Theory of differential association*

1b. *Theories of subcultural transmission* (including ecological studies).

Among role-self theories we can distinguish:

2a. *Labelling theory* (including scapegoat-theory)

2b. *Stigma theory*

2c. *Secondary deviation theory*

(These three are greatly overlapping.)

Lastly there are eclectic theories like

4. *Sociological reaction-formation theory*

5. *Theory of differential opportunity, namely, differential access to illegitimate means*

6. *Differential identification theory*

7. *Containment theory*

(This classification could of course have been made otherwise too. I chose it as just one possible means of structuring the field).

I shall try to expound the explanations these several theories have to offer as succinctly as possible and, if necessary, the criticism they have raised.

5. The founding father of cultural transmission theory in criminology is E. H. Sutherland[16] who formulated the theory of differential association (1a). His main thesis, in sharp contrast to the views of say Hobbes, Lombroso or Freud was: 'Criminal behavior is learned.'[17] (The philosophical basis of this and other cultural transmission theories is probably to be found in John Locke's concept of man as a *tabula rasa* at birth. If these sociologists did not read Locke, they certainly read W. James, the

psychologist, through whom Locke's view may have been culturally transmitted.)

The same social processes that make us learn conforming behavior are responsible for our learning deviant behavior. 'Criminal behavior is learned in interaction with other persons in a process of communication.' (The communication need not be of a verbal type nor of a face-to-face type, writes Cressey in response to critics of the theory.) The learning mostly happens within intimate groups and includes both (a) techniques of committing the crime and (b) the specific direction of motives, attitudes, rationalizations.

One learns to define the norms of the legal code as favorable or unfavorable and a person then becomes delinquent because of an excess of definitions favorable to violation of law over definitions unfavorable to violation of law.

It may be clear from the above that Sutherland tried to be clear, unambiguous, almost mathematically exact and strictly sociological in his explanation of criminal behavior and that his work was a sharp reaction to the kind of explanations we have met among psychodynamic criminologists. Some of his concepts were rather rough, others were ambiguous. Cressey[18] has done much to clarify the theory and has answered the critics of it.

A. J. Reiss, Jr. and A. L. Rhodes tested it empirically [19]. The test was not very favorable for the theory in its classic form, but lent some support to younger theories inspired by it or anyway in the same vein of (sociological) thought.

6. C. R. Clifford and H. D. McKay developed a cultural transmission theory from ecological research.[20] They were trained in the so-called *Chicago School* in sociology to which W. I. Thomas and F. Znaniecki,[21] G. H. Mead[22] and E. W. Burgess[23] made important contributions.

Shaw and McKay tried to arrive at an explanation for the distribution of crime in 20 American cities and specifically Chicago. It struck them that high rates of delinquency for certain areas of Chicago remained constant through time (periods analyzed were 1900–1906 and 1917–1923), notwithstanding the changing population of these areas in which ethnic groups came and went. Parallel with the constant rate of crime per area they noted that the crime index for the migrating ethnic groups rose as they entered the area and fell as they left it.

Further they remarked that most delinquents operated in small groups

and that criminal conduct norms had become established social norms in these areas. Agents of transmission of the deviant (subcultural) norms were playgroups of children and gangs.

They stress the fact that in these particular areas (young) people are satisfying the same social needs as ordinary people in non-delinquent areas. They satisfy their need for excitement, security, companionship and such, only the means are deviant.

Critics of their theory have suggested that differences in crime rates might be accounted for by differences in police reporting and convictions. Others thought that areas known for a high rate of delinquency might be attracting already delinquent (or thus inclined) immigrants.

Empirical evidence collected by C. F. Schmid[24] confirms differences in reporting but analysis shows that differences in crime rates cannot be accounted for solely on this ground. For the ecological 'drift theory' there is little evidence too. Evidence to the contrary has been gathered with respect to mental disorder (specifically schizophrenia) for which a similar hypothesis had been suggested. (R. E. L. Faris;[25] H. W. Dunham[26]). Disturbed people are not drifting towards slums, but slums create disturbed people, is the overall conclusion.

7. Another important theory of cultural transmission was given by W. B. Miller.[27] He explained that gang delinquency (numerically by far the most important form of delinquency)[28] was a product of lower class culture. The behavioral norms of this culture he described in detail. The habit of staying outdoors in one-sex peer groups, the rhythm of daily life between dull routine plus concern for 'staying out of trouble' and looking for excitement through thrill, risk, danger.

Further the highly valued qualities of 'toughness' and 'smartness' and the all pervasive feeling that what happens to you is dependent on whether you are 'lucky' or 'unlucky', not so much on your own activities.

From his vivid and probably very accurate descriptions Miller drew some conclusions that are the opposite of explanations found in most psychodynamic explanations, anomie theory, most variants of role-self theory and the eclectic 'reaction-formation' theory of Becker. (For an exposition of the contents of the role-self class of theories and of Becker's theory see below).

Miller stated that, contrary to the belief of others, lower-class boys do not become delinquent out of frustration and aggression against those who

have better socioeconomic chances, but that they are striving for the ordinary goals of 'belonging' and 'status' via subculturally approved means (like 'toughness' and 'smartness').

He even denied that there were fundamental differences between those street corner groups who did and who did not violate the law. The same norms and values, the same goals and means reigned pervasively in the whole lower class culture. If some adolescent groups happened to become lawbreakers this was rather accidental. The lawbreaking groups could not be seen as a quite distinct entity.

Important is Miller's contention that male lower class street corner groups behave as they do because they need a reference group for male identification (lower class households being typically female based), that they act as they do for pleasure (belonging; status) and kicks, not out of a grudge felt towards middle class culture, and that the different outlets they seek for quite ordinary strivings are determined by lower class subculture as a whole, not by specific illegal norms (norms in conflict with those of penal law). Thus Miller clearly belongs to the same school of thought as Clifford and McKay and Sutherland and Cressey, although he is much more explicit on several points.

Miller has been criticized by D. Bordua.[29] According to the latter there is danger of tautology in Miller's explanation. Miller derived the focal concerns of lower class behavior from observation of lower class street corner groups and then used these focal concerns to explain the behavior of the group, he says. (He ought to have taken external, objective variables, such as prolonged instability of employment, as independent variables.) Further Miller is slighting the impact of middle class values on lower class culture too much (and thus underestimating possible sources of frustration and aggression).

But Bordua admits that Miller did good work by telling what lower class boys are for, instead of accentuating what they are against, and in explaining the impact of adult culture on adolescent behavior. But what Miller really described, according to his critics, was urban slum behavior and norms, not all of lower class culture, and of course his theory had little or nothing to say about middle class delinquency.

8. Other writers have tried to explain delinquency, or delinquency of middle class youth, and also adult criminality, from the influence of some dominant traits in the (American) culture:

1. Belief that everyone has a racket
2. The search for something for nothing
3. Growing acceptance of violence
4. Discrimination against minorities[30]
5. Popular reliance on punishment as a deterrent
6. White-collar crime
7. Misrepresentation in advertising

and so on.[31]

The difference between these explanations and anomie theory is their much greater differentiation among the variables allegedly the causal ones for crime. The difference between them and studies like those of Shaw and McKay or Miller is that they are mostly based on prescientific hunches, not on empirical research.

My criticism: some of the allegedly causal variables are already implying criminality themselves (e.g. 4; 6; 7). Where did they come from?

9. Rationalizations or techniques of verbalization for delinquent behavior plus the confrontation of boys with an inconsistent legal system and its agents have been given as an explanation for 'drift into delinquency' by G. M. Sykes and D. Matza.[32] From incidental lawbreaking behavior a boy may drift into more serious delinquency through the processes mentioned, become a member of a still more serious adult criminal gang (the membership of which is chiefly recruited, it seems, from adolescents who have had a record of incarceration in correctional institutions) and finally become associated with organized criminal rackets.

This sociological reinforcement theory (on the verge between the sociology of penal law and the sociology of deviant behavior because of the influence it stresses of confrontation with agents of the law, conviction and correction upon future criminal behavior) merits much attention from empirical researchers. Wilkins[33] has stressed the same point from the opposite perspective: the best thing to do with a delinquent in order to prevent recidivism is probably doing absolutely nothing (not even stigmatizing him through well-meaning counseling or such) at the same time telling the public (for general prevention) that he has been terribly punished...

10. Role-self theory has been established by G. H. Mead,[34] a sociologist also from the University of Chicago. Application of his theory to deviant behavior has led to the concepts of (subcultural) reference group, role-con-

flict (or role strain), secondary deviation, scapegoat criminals and (partly at least) labelling and stigma.

The concept of *self* (and its dependence upon the perception of other people's behavior, the interpretation and response of it) must in first instance have been derived from philosophical anthropology (man as the only being on earth that has a self, etcetera).

The concept of role has clearly been derived from the theatre.

Shakespeare's:

'All the world's a stage
And all the men and women merely players;
They have their exits and their entrances;
And one man in his time plays many parts,'

may have been the inspiration for it.[35]

Sociologists using the role-self or role-status twin concepts, or speaking solely of role-performance do not want to imply that these social roles should be something alien to our own individuality like arbitrary roles played in the theatre. In fact the sum total of all the social roles that we feel inclined or are obliged to play constitute together our individuality.[36]

Social behavior is learned: it develops from birth onward through interaction with others, through 'socialization'. Demands and expectations of others make us modify our already acquired behavior. So there is a constant interaction between actor and others.

The activities of deviants as well as non-deviants can be seen as responses to actual demands and expectations of others but also as responses to anticipated (or 'projected' that is subjectively assumed) expectations of others.

Whatever roles we learn to perform during infancy and youth, e.g. sex roles, is largely determined by the social structure and cultural norms of our environment as cultural anthropology has demonstrated.[37]

Role strain or role conflict arises in a situation with complex role demands or where a person is (feels) required to fulfil multiple roles that may contradict each other. Or the role prescriptions may be unclear and so make the actor insecure.

T. Parsons[38] has taken role conflict as the basic explanative variable for deviant behavior.

Other sociologists, like A.K. Cohen[39] have explained deviant behavior not so much as dependent on role conflict but simply as the expression of roles. The man who drives a car too fast need not be in a hurry but may want to express 'that kind of a person am I'. The same goes for the use of mari-

huana, open rebellion against authorities, ostentatious sexual activity and promiscuousness, even suicide. It may all be role-expressive behavior more than anything else.

Besides role-expressive behavior (which may happen to be deviant), one may distinguish role-supporting behavior.[40] A boy does not need to steal in order to express his masculine role, but under circumstances the stealing (or other risky activities) may express that role, make it more convincing, at least in his own eyes and that of his reference group.

For most girls in our culture, stealing cannot be role expressive (she ought to be sweet, beautiful and passive for that), but it could be role supporting if she stole beautiful clothes, perfume and jewels.

11. Clinard[41] writes: 'The status and role (or roles) which a person is assigned cannot be easily changed by his own desires: whether a person plays the role which society has assigned to him or not, his behavior is still interpreted by society as consistent with his role and its corresponding status.'

This means that 'once a thief, always a thief' may be a deep wisdom, but from another point of view than most people tend to think. Sociologists who became aware of the rigid role assignments we give each other have coined concepts like 'labeling', 'stigma' and 'secondary deviation' in connection with this. These concepts are otherwise fit just as well for describing the situation of mental patients as for explaining (continued) criminal behavior.

One of the first criminologists to draw attention to societal reaction upon the criminal instead of focusing on the latter's character or individual social background was F. Tannenbaum:[42] 'It [society] proclaims his [the criminal's] career in such loud and dramatic forms that both he and the community accept the judgment as a fixed description.' ... 'the community expects him to live up to his reputation, and will not credit him if he does not live up to it.'

H. S. Becker formulated the labelling process as follows (*Outsiders*, p.9): 'Deviant behavior is a consequence of the application by others of rules and sanctions. The deviant is one to whom the label has succesfully been applied; deviant behavior is behavior that people so label.'

This rigid role-prescription and role-acceptance (also called self-fulfilling prophecy) serves as an explanation for much deviant (criminal) behavior.

E. M. Lemert added the concept of secondary deviance to it:[43] 'When a person begins to employ his deviant behavior or a role based upon it as a means of defense, attack or adjustment to the overt and covert problems created by the consequent societal reaction to him, his deviation is secondary.'

The labelling process is made effective through 'successful status degradation ceremonies' such as the penal process and other proceedings in connection with crime (arrest; probation reports and such). H. Garfinkel[44] writes: 'The other person becomes in the eyes of the condemners literally a different and new person' ... 'the former identity stands as accidental;' ... 'What he is now is what "after all", he was all along.'

'Stigmatization' as an explanative variable both prior to prosecution and as a consequence of it has been stressed both by E. Goffman[45] and D. H. Chapman.[46] The latter thereby drawing attention to society's need of collective scapegoats.

The whole process of creation of deviance and of criminalizing deviance through societal reaction to behavior that is not harmful *in se* ('crimes without victims' like homosexual relations between consenting adults or smoking marihunana) has been described by E. M. Schur.[47] He writes: 'As already noted, crime or other deviance promotes solidarity in the group. It may serve psychological functions for conforming individuals, and may also serve quite definite economic functions which may – in turn – give rise to a variety of vested interests in its continuation (on the part of specialized law enforcement agencies, for example, as well as on the part of professionalized offenders).' (We should be aware that not only the violent, aggressive, or outright sadistic law enforcement agencies may have a vested interest in the continuation of crime but also humanitarian agencies that are there to guide and help the criminal. A striking example of the latter are Child Welfare agencies, most of whose activities (institutionalization; stigmatizing) have been proved to be most harmful for children, even if from none too good backgrounds. Yet these agencies are there to stay with us. C.I.D.).

12. The concept of *interaction* (which lately has become rather fashionable) can be used in connection with almost any theory. Cohen[48] writes: 'Interaction process implies that an actor's initial move alters his environment and thereby the conditions that affect his next move.' Thus, the con-

cept needs some conceptual framework from some theory to become substantial.

In connection with eclectic theories that try to combine psychodynamic with sociological models its use may be evident: psychological variables are taken as the independent ones, the societal reactions they evoke and the actor's reaction to these societal reactions as intervening variables for the eventual criminal behavior. (Below, when the conditions for causality will be discussed, I shall indicate why this application of the interaction concept with regard to criminality is untenable in my opinion: neither is there a high association between specific psychological variables and crime in general, nor is it always possible to prove the necessary causal order, nor is there lack of spuriousness.)

The other way round one could use the interaction concept in eclectic theories that take sociological variables as the independent ones, psychological variables (psychological reactions to the social situation) as necessary intervening ones. This position is tenable in principle (see below). Also this position is roughly one that disciples of anomie theorie might take: the prescribed cultural goals are there for everyone but it depends upon one's definition of the situation and further psychological reactions to the latter whether or not one will resort to illegitimate means in connection with these goals.

In combination with role theory interactionism will stress the give-and-take process of acquiring roles and becoming committed to them. Especially in combination with the conceptual framework of secondary deviance it may be useful.

The combination of differential association theory as a specialized type of cultural transmission theory with interactionism is indicated by Cohen[49] as follows: ... 'we would attempt to formulate the responses of the milieu in terms of (a) transformations in the content of the milieu itself, including the impact upon the milieu of *its* associations with the actor and (b) changes in those properties of the relationship between the actor and his milieu affecting the frequency, priority, duration and intensity of association with pro-deviant and anti-deviant associations'.

The 'definition of the situation' which has been stressed as an important 'interactionistic' intervening variable from the individual's point of view (Shibutani)[50] can also be used to explain the behavior of groups or institutions that create or enforce penal law.

For an example of the combination of interactionism and cultural trans-

mission theory via the 'definition of the situation' by other people than the delinquent actor see Short and Strodtbeck.[51] They imply that delinquent subcultures may lead to delinquency not primarily by what the actors do, but through the reactions of others to acts that are still pre-delinquent *in se*.

13. Among the eclectic theories that I want to mention is what I have coined 'sociological reaction-formation theory'. It may be seen as a theory that conjuncts concepts from role theory (especially in its versions as labelling theory and theory of secondary deviation) with the concept of interaction but also the psychodynamic concept of overcompensation.

The divisions made in paragraph 4 of this chapter manifest their artificiality and overlap also in this case. Short and Strodtbeck, *op. cit.*, although not using the term reaction-formation, in fact describe a process in their cultural transmission theory that might very well be so defined (see again note[51]).

Cohen[52] explicitly uses the concept: 'The dominant value system is also, to a degree, *their* [the lower-class boys'] value system. They have, to a certain extent, internalized its rules also. They can *tell* themselves that they don't really care' ... 'but their internalized values, even if repressed, threaten always to break through' ... 'Therefore, to buttress this choice, to protect it from incursions from "the enemy within as well as the enemy without", they resort to reaction-formation.'

Cloward and Ohlin's theory of differential opportunity (differential access to illegitimate means)[53] eclectically combines elements from cultural transmission theory, role theory and anomie theory,

The concept of restricted legitimate opportunities stems of course from the anomie tradition. The access to illegitimate opportunities, however, is also unequally distributed, according to whether or not one has access to a subculture, they say. Here we have the cultural transmission element. Not everyone has the same opportunities to learn, to practice and to perform deviant *roles*. (The third theoretical basis.)

On the basis of their opportunity concept they distinguish three types of delinquent subcultures that are all three solutions for the strain noted by Merton:

a) the criminal subculture (well-organized crime for economic gain)
b) the conflict subculture (violence and gang fighting)
c) the retreatist subculture (drug use and other 'kicks')

14. Glaser[54] has constructed what he himself calls 'an integrative criminality theory'. Identification is defined by him as 'the choice of another, from whose perspective we view our own behavior'. He reconceptualizes Sutherland's theory in role-taking imagery 'drawing heavily on Mead as well as on later refinements of role theory'. But since role theory is also suitable for combining sociological and psychological models, his theory (which has not adequately been tested as far as I know) might be called integrative to the second degree.[55]

15. Traditional monistic causal explanations for crime in general (of which many have been offered before 1940) were so contradicting and the empirical evidence for them so vanishingly small that several authors have suggested to leave the quest for causes in criminology and concentrate on non-causal research. Radzinowicz[56] expressed that opinion in 1960, but already in 1940 Reckless[57] had been suggesting the same.

Wootton[58] has enumerated twelve commonly investigated variables in regard with crime:

1. the size of the delinquent's family
2. the presence of other criminals in the family
3. club membership
4. church attendance
5. employment record
6. social status
7. poverty
8. mother's employment outside home
9. school truancy
10. broken home
11. health
12. educational attainment

She examined 21 relatively well done empirical studies that tried to test the impact of one or more of these factors and found them all unconvincing. Although her reasoning *why* these studies are not convincing with regard to causal relationship is in itself invalid[59] one must admit that all of these factors (either taken as a monistic explanation or combined) are not very impressive when put to empirical test. From a logical point of view it is evident why this is so:

a. the impact of penal law and its enforcement, of labeling and stigmatizing is totally ignored by this type of study;

b. all crime is taken as if it were one category of behavior. (See Chapters I and IV above on these points).

Reckless, feeling frustrated about this situation in criminology, published an article in 1962[60] in which he attempted to give a 'non-causal explanation': *containment theory*.

Reckless states: 'The assumption is that there is a containing external social structure which holds individuals in line and that there is also an internal buffer which protects people against deviation of the social and legal norms.' ...'If there are "causes" which lead to deviant behavior, they are negated, neutralized, rendered impotent or are paired by the two containing buffers.'

Among the inner containment are listed by him variables from role theory: 'a favorable image of self in relation to other persons, groups and institutions'; from frustration-aggression theory (that is itself a combination of behavioristic and Freudianistic models): 'a high level of frustration tolerance'; from Freudianism: 'well developed ego and super ego'.

Reckless supposes that sociologists will be able to measure the outward, psychologists the internal containment of any individual.

In my opinion Reckless greatly overestimates the operationability of his theory (in fact it is a game theoretic model, admitting only qualitative assessments, not an exact weighting of his several factors, if only because scales used by sociologists and by psychologists cannot be compared since they do not measure in the same units).

Also his conception of non-causality is in fact identical, it seems to me, to a complex multiple causation theory.

16. To assess the several explanative theories that are current in criminology today, it is essential to make some remarks first about the concepts of causality, multiple causation and some false criteria of non-causality that are often applied in criminology.

According to Hyman[61] and to Hirschi and Selvin[62] who agree with him, all statistical analyses of causal relations in delinquency rest on (1) observed associations between the independent and the dependent *(in casu* delinquency in one form or another) variables. This is the first criterion of causality. Two additional criteria are: (2) the independent variable must be causally prior to the dependent variable and (3) the original association must not disappear when the influences of other variables causally prior

to both of the original variables are removed. Thus we have briefly these three criteria which must be met in order that we may assume causality:

1. association
2. causal order
3. lack of spuriousness

It will be clear that only the first criterion is relatively easily met, the second is harder and the third is in fact never met in behavioral science, although there may be a high degree of probability that it has been met.

In order to prove statistically a causal relationship one has to prove, or make highly probable, that a relationship between some independent variable(s) and the dependent one is in accordance with these *three* criteria. In order to disprove that the relationship is a causal one, the only thing necessary is to falsify the assumption for but *one* of these criteria (or make highly probable that it has been falsified).[63] So it is much easier to reject causality than to prove it.

The important analysis of Hirschi and Selvin with regard to causality shows that notwithstanding the relative easiness of rejecting causal assumptions, many if not most of these rejections in criminology are made on the basis of *false* criteria of non-causality. They exemplify six of these false criteria:

1. Insofar as a factor is not a necessary or is not a sufficient condition for delinquency, it is not a cause of delinquency.

(A non-perfect association between the variables is, illegitemately, not accepted).

2. Insofar as a factor is not characteristic of delinquents, it is not a cause of delinquency.

(Instead of looking for significant differences in the association between independent and dependent variables for delinquents and non-delinquents, one looks, wrongly, if the factor is present in a high percentage of cases among delinquents).

3. If a relation between an independent variable and delinquency is found for a single value of a situational or contextual factor, that situational or contextual factor cannot be a cause of delinquency.

4. If a relation is observed between an independent variable and delinquency and if a psychological variable is suggested as intervening between these two variables, then the original relation is not causal.

5. Measurable variables are not causes.

6. If a relation between an independent variable and delinquency is

conditional upon the value of other variables, the independent variable is not a cause of delinquency.

The reader who finds it hard to interpret these false criteria of non-causality in concrete criminological terms is referred to Hirschi and Selvin for examples from the criminological literature. Their brilliant analysis has my full agreement except that, in my opinion, one should evade causal models as much as possible in fundamental science, exactly since they are so hard to make reasonably probable, so easy to refute or make refutation highly probable. (The latter should be done on one of the three legitimate grounds of course, and should not be based on one of the false criteria of non-causality exposed by the authors.)

But further I can quite agree with their attack on those who think (implicitly often) that only mono-causality is 'true' causality, thus denying the possibility of multiple causation (to be measured as multiple or partial correlation in the first instance).

Wilkins[64] when attacking what he calls *theories* of multiple causation is making a mistake because of a word (the ample misuse of the term 'theory'). The causal model (multiple or not) is used in connection with statistical experiments at the pre-theoretical level of scientific inquiry and precedes eventual theoretical interpretation. Wilkins' objection to this 'theory' that it cannot be falsified is false in two ways:

a. causality as a model cannot be falsified, nor should it be, unless one wants to reject it and replace it by some other model that seems more isomorphic to the data studied;

b. statistical proof of (multiple) causality can, in principle, easily be rejected (see above).

If there are people around in criminology who speak of the model of multiple causation or of its statistical proof as if it were a theory in the true sense, that is their fault of course, but Wilkins should have attacked them for misuse of terminology, not for their inept 'theory'.

17. Of the main types of criminological theories that have been indicated above, the psychodynamic ones, insofar as they claim psychological variables to be the independent ones, social variables to be of no relevance or at best to be relevant as intervening variables, have been disproved by empirical research.

In fact it are (clusters of) sociological variables that are the independent ones, psychological variables that are intervening ones. And it would be

application of false criterion of non-causality number 4, if one denied the causal impact of the sociological variables on this ground.

Biological explanations may be valid in some cases, only one will need long chains of intervening variables (or constant social boundary conditions, like the penal law enforcement) to really 'reach' the dependent variable: criminal behavior. This, however, does not impair their causal influence.

Of the sociological explanations, anomie theory is the most vague, least operational of all. It has been argued in criminology, e.g. by Lander[65] that hard, measurable factors like 'broken homes' are only *indicators* of the true cause of delinquency: anomie. This means, as Hirschi and Selvin have already pointed out[66] that objective measurements are exchanged for a vague term, the indicators of which are: these measurements. Thus a really unscientific, unfalsifiable statement is made.

It is hard to imagine how anomie should be measured otherwise than by 'hard' variables that are traditionally associated with crime and deviance. It is a typology (and a typology with logical defects; see Chapter II above) that Merton made, but its use for empirical research seems small.

On the other hand, cultural transmission theories, especially in Cressey's more adequate version of Sutherland's original theory of differential association is much easier made operationable and indeed some experimental research based on its assumptions has been done.[67] Its defect lies in the accent it puts on becoming a member of a subculture or the continuation of the subculture, while it fails to explain how a subculture comes into being at all.

For the genesis of subcultures one has to read Cohen or other eclecticians. But: Cohen's idea of the rancorous, frustrated lower-class has been very severely and convincingly attacked.[68]

Labelling and stigma and drift theories explain several things but tend to overstate their point. The active role of some actors that makes them apt for labelling and stigmatizing and drifting is underestimated.

Role-theory in general underestimates the individual's independence of group processes. Man is seen as a social robot: an oversocialized concept of man (in contrast to the over-instinctual view of man by Hobbes, Freud and Lombroso) lies at the roots of it, it seems.

18. Where we stand at present in criminological explanation is hard to say. One of the most important recent contributions to (future) explanation is

probably Hirschi and Selvin's searching analysis of causal research and misaimed criticism of it.

There is ample evidence by now for what one should not use in criminology: overlapping, unexhaustive or otherwise failing typologies. Psychodynamic models. Biological explanations without specifying the chain of intervening variables that links the biological datum with behavior defined as criminal. Unspecified anomie theory.

Most promising seem to be those cultural transmission theories that explain both the genesis of subcultures and the process of becoming a member of them: the learning process that leads to eventual participation, and finally the reaction of society towards members of such a subculture and the consequences thereof.

In terms of existing theories this means that – irrespective of their actual defects – one would need a theory that explains the origin of subcultures in the way the theories of W. B. Miller or A. K. Cohen try to do that. In addition it should contain a sociological learning theory of the type that Sutherland and Cressey developed. Further the labelling and stigmatizing process should be described and explained without, however, overemphasizing this point as is usual in the present literature of the 'labelling' school of thought.[69]

Lastly the feedback from societal reaction on (delinquent) subcultures should be described and explained.

NOTES

1. C. Lombroso, *Crime: its Causes and Conditions*, Boston 1918 (1876). Lombroso's work has been used by those who reacted against the Classical School in prescientific criminology. (The latter was based on a speculative model of man inspired by the Enlightment: man as a strictly rational, free being, rationally maximizing his profits and thus to be deterred from crime by heavy sanctions). Lombroso thought of criminal man as an *atavistic* being (the evolutionary concept stems of course from Ch. Darwin whom he admired greatly), hardly responsible for his instinctual, 'beastly' drives. (Animals and human forefathers were supposed by him to be of an anti-social character. For higher mammals this certainly is not true, as ethologists have demonstrated.) Although his views on man, animals, primitive man and criminal man were highly romantic, contrary to (later) evidence and sometimes outright absurd, Lombroso took the great step of doing empirical (anatomical) research on criminals, at a time when priests and lawyers were thought to be the most competent people to explain criminal behavior.

Still into the twentieth century Lombroso dominated European criminology until Ch. Goring refuted his conclusions in his book *The English Convict*, London 1913. Goring investigated control groups alongside large samples of English prisoners and found no significant differences on Lombroso's variables.
According to Clinard, *op. cit.*, E. A. Hooton, *The American Criminal: an Anthropological Study*, Cambridge, Mass. 1939 established against Goring's evidence that criminals differ significantly in anthropological attributes from non-criminals. He took tremendous samples and control groups (nearly 2,000 people in total) and measured them on numerous anthropological variables. He found that not only the control group differed significantly from the criminals, but also the latter among themselves according to type of crime committed. In 1947 H. von Hentig made a similar claim for redheads who were supposed by him to be excessively criminal. (Refuted by Ph. J. Rasch. Both articles in *Journal of Criminal Law and Criminology*, 38, 1947). Hooton, despite his vast samples, had forgotten to do some critical matching. In fact he compared prisoners with bathing resort guests (in the thirties!) firemen, state militiamen and hospital out-patients.
E. Kretschmer, *Physique and Character*, London 1925 distinguished three basic somatotypes, supposedly correlated with types of mental illness. A rather analogous tripartition was constructed by W. H. Sheldon. See his *Varieties of Delinquent Youth*, New York 1949.
According to Sheldon delinquents were mainly heavy, insensitive, aggressive 'mesomorphs'. In the same vein has been done the research collected in Sh. and E. Glueck, *Physique and Delinquency*, New York, 1956. Sheldon has been criticized by E. H. Sutherland: 'A Critique of Sheldon's Varieties of Delinquent Youth', *Am. Sociol. Review*, 16, 1951, pp. 10–13.
A more extensive survey of biological explanations of criminal behavior (including endocrinological studies and twin-studies) is to be found in R. R. Korn and Ll. W. McCorkle, *Criminology and Penology*, New York 1959, Chapter 10.
In general, biological studies on criminals have been statistically false or inconclusive. Although these attempts at explanation are much more interesting from a scientific point of view than others more well known, like the shoddy psychodynamic ones, much research remains to be done. But whatever will be the research results: a long chain of intervening variables will have to be accounted for since crime is a socially defined type of behavior.

1a. J. J. M. van Dijk, *Relevantie van de ethologie van de mens voor politieke en rechtswetenschap*, 1970.

2. There are a few good publications in the field of sociology of law by now, but very little of it contains the results of empirical research that may be useful for criminology. See: B.-M. Persson Blegvad, (ed.).: 'Contributions to the Sociology of Law', in: *Acta Sociologica*, 10, 1–2, 1966.
Further: V. Aubert, (ed.), *Sociology of Law*, Harmondsworth 1969. L. M. Fried-

man and S. Macauley, *Law and the Behavioral Sciences*, New York 1969. J.H. Skolnick, *Justice Without Trail; Law Enforcement in a Democratic Society*, New York 1966.

J. Stone, *Social Dimensions of Law and Justice*, New York 1966.

3. A.K. Cohen, *Deviance and Control*, Englewood Cliffs, N.J. 1966, p. 42

4. L.T. Wilkins, *Evaluation of Penal Measures*, New York 1969, p. 31.

5. D. Feldman, 'Psychoanalysis and Crime', in: B. Rosenberg, et al., (eds.), *Mass Society in Crisis*, New York 1964, pp. 50–58. For another lucid discussion of deviant behavior theories focussing on the individual, particularly the psychoanalytic ones, see M.B. Clinard, *Sociology of Deviant Behavior*, New York 1968[3] (1957), pp. 174–189. Clinard mentions some other variations on the psychodynamic main-theme, comes roughly to the same points of criticism as made by me and adds some more. I shall briefly summarize them in my own words in the following nine points:

1. Empirical research favors the hypothesis that human behavior is a product of social experience and that it is not determined by an innate reservoir of animal [supposedly anti- or a-social, C.I.D.] impulses termed the *id* [another term for the *Es*, C.I.D]

2. The psychoanalytic emphasis on sexual eroticism is a great overstatement of an important aspect of human behavior.

3. Contrary to psychoanalytic belief, the childs development is greatly influenced by social relationships which have little or no connection with bodily functions. Oral, anal and genital experiences are grossly overemphasized as decisive influences on development.

4. Psychoanalytic theory has assumed, contrary to now available empirical evidence, that there are certain uniformities in human behavior based on biological drives, irrespective of cultural influences, historical eras, or variations in social structure. E.g. the cross-sex parental preference (the so-called Oedipus relationship) is not universal across cultures but if it exists, it is a product of the child's social relationships.

5. Psychoanalysis is not a scientific explanation. Most of psychoanalysis has not been verified, most psychonanalysists are not aware of the possible type of evidence that would refute their theories, and 'the theory is stated in a language so vague and metaphysical that almost anything seems compatible with it'. (Quotation by Clinard from E. Nagel, 'Methodological Issues in Psychoanalytic Theory', in: S. Hook (ed.), *Psychoanalysis, Scientific Method and Philosophy*, New York 1964.)

6. Psychoanalytic as well as psychiatric theory make an unwarranted reduction to early childhood experiences, mainly in the family, whereas empirical evidence shows overwhelmingly that situations and social roles determine variations in behavior and that personality continues to develop throughout life.

7. Despite their claims, the explanations of psychiatrists and psychoanalysists and their proponents concerning *deviant* behavior have not been verified. They have even often refused to use experimental or more verifiable situations, or other more rigorous and controlled techniques, to test their hypotheses. Their 'evidence' is mostly based on extremely few cases that happened to have come to their attention (e.g. by asking for treatment!).

8. Psychiatric and psychoanalytic explanations tend to blur the difference between *sickness* and deviance. Criminal or antisocial behavior is supposed to be the criterion for mental abberration. Cf. the old criminological 'theory' that criminals were possessed by devils, or the idea that criminals are persons who are *guilty* (W. H. Kempe, cited in a different context in the *Preface* of this book), or '*failing*' (H. Bianchi, see note 4 of the *Preface*).

9. Not only has hardly any psychoanalytic statement been validated, also these statements are unreliable. There is great absence of agreement among psychoanalists about what would constitute objective criteria for diagnosis (in treatment) or explanation (in science). See for this B. Wootton, *Social Science and Social Pathology*, New York 1960, Chapter VII, pp. 204–226.

6. See K. Horney, *New Ways in Psychoanalysis*, London 1954[3] (1939).

7. F. Alexander and H. Staub, *The Criminal, The Judge and the Public*, Glencoe, Ill. 1956[2] (1931).

8. W. Bromberg and C.B. Thompson, 'The Relation of Psychosis, Mental Defect and Personality Types to Crime', *Journal of Criminal Law and Criminology*, 28.

The authors studied a random sample of nearly 10.000 convicted criminals. They found that 82.0 per cent were 'average or normal.'

For a review of the older research literature see L. G. Lowrey, 'Delinquent and Criminal Personalities', in: J. M. Hunt (ed.), *Personality and the Behavior Disorders*, Vol. II, New York 1944. Further: K. F. Schuessler and D. R. Cressey, 'Personality Characteristics of Criminals', *Am. J. of Sociol.*, 55, 1950.

Also: M. B. Clinard, 'Criminological Research,' in: R. K. Merton, et al., (eds.), *Sociology Today*, Vol. II, New York 1965 (1959), pp. 521–526.

Eysenck's personality theory of crime (of course not a psychoanalytical but a strictly psychological one) has already been mentioned (plus the criticism and falsifying tests it received) in Chapter II.

9. See K. Friedlander, *The Psychoanalytical Approach to Juvenile Delinquency*, New York 1945.

10. See for example W. Healy and A. Bronner, *New Light on Delinquency and its Treatment*, New Haven, Conn. 1936.

11. See A. M. Johnson, 'Sanctions for Superego Lacunae of Adolescents', in: S. Arieti (ed.), *American Handbook of Psychiatry*, Vol. I., New York 1959.

12. There are of course neo-Freudians who are socially critical, 'progressive'

people, and there are socially critical, 'progressive' people who believe in Freud. If they were fully aware, however, of the social implications and the ideological background of Freudianism, they would get into a serious role-conflict.

13. F. Alexander, *Our Age of Unreason*, or F. Alexander and W. Healy, *The Roots of Crime*, New York 1935 (the conclusion in this book are based on *six* (6) case-studies!).

14. See note 8 above. Schuessler and Cressey *op. cit.*, analyzed 113 studies up to 1950 that had tested the relationship between personality traits and crime. Their conclusion: 'The doubtful validity of many of the obtained differences, as well as the lack of consistency in the combined results, makes it impossible to conclude from these data that criminality and personality elements are associated.' They were criticized by Vold for taking together well and badly conducted research. See G.B. Vold, *Theoretical Criminology*, New York 1958, p. 216. However, most evidence is in favor of the hypothesis that there is no association between personality traits (regardless of the scale they are measured on, from a rough extraversion – introversion scale to the MMPI (Minnesota Multiphasic Personality Inventory) which contains 550 statements which the subject is asked to respond to as being true or false about himself, to projective tests like the TAT (Thematic Apperception Test) or the Rohrschach). Even the best of these tests, which is probably the MMPI, measure things that have been proven to be most probably not significantly related to criminality!
Especially in Europe where so many explorative, descriptive research is undertaken (in contrast to validating research) personality variables may be tested and taken down as a matter of routine when doing criminological research. This implies that these researchers still think this kind of variables to be relevant for future theoretical interpretation. However, unless they are able to prove the contrary, this is not the case. Their time-consuming, costly enterprise is most probably useless for arriving at theoretical insights.
As for the concept of 'psychopath' (a pseudo-variable allegedly related to crime) this has already since about 1950 been recognized as unfit for empirical research (the circular reasoning it involved was mostly that one was a criminal because he was a psychopath, and a psychopath because he was a criminal. See Wootton, *op. cit.* on this subject, pp. 249–251).
Most modern psychiatrists know that the term has been discredited (it is very much analogous to the '*moral insanity*' of former times) and will evade it.

15. E. Durkheim, *Suicide*, Glencoe, Ill. 1966 (*Le Suicide*, *Paris* 1897).

16. E.H. Sutherland and D.R. Cressey, *Principles of Criminology*, Chicago 1966 (1924).

17. For a metasociological analysis of the two opposite concepts of man that are at the roots of sociology of deviant behavior see C.I. Dessaur, 'De sociologische mens', in: R. Wentholt (ed.), *Het mensbeeld in de wetenschappen*, Rotterdam 1971.

The Hobbes – Lombroso – Freud stream is contrasted with the Aristotle – Scheler – Sutherland stream.

18. D.R. Cressey, 'Epidemiology and Individual Conduct: A Case from Criminology', *The Pacific Sociological Review*, 1960, 3, pp. 47–54.
He remarks: 'The statement of the theory of differential association is not clear. In two pages, Sutherland presented nine propositions, with little elaboration, that purport to explain both the epidemiology of crime and delinquency and the presence of criminality and delinquency in individual cases.' ... 'Most significantly, as we shall see later, the statement gives the impression that there is little concern for explaining variations in crime and delinquency.'
Cressey elaborates what Sutherland meant and attacks his (many) critics for five principal errors on their part and some minor ones. It would lead us too far to follow his argument here.

19. A.J. Reiss, Jr. and A.L. Rhodes, 'An Empirical Test of Differential Association Theory', *Journal of Research in Crime and Delinquency*, 1964, 1, pp. 5–18.

20. C.R. Shaw and H.D. McKay, *Juvenile Delinquency and Urban Areas*, Chicago 1942.

21. W.I. Thomas and F. Znaniecki, *The Polish Peasant in Europe and America*, New York 1927.

22. G.H. Mead, *Mind, Self and Society*, Chicago 1934.

23. E.W. Burgess, (ed.), *The Urban Community*, Chicago 1926 (herein: W.I. Thomas, 'The Problem of Personality in Urban Environment').

24. R.E. Park and E.W. Burgess, *The City*, Chicago 1925. C.F. Schmid, 'Urban Crime Areas: Part II', *Am. Sociol. Review*, 1960, 25, pp. 655–678. Schmid writes: 'In spite of crime statistics and distortion in the derivation of rates resulting from differentials in population mobility and composition, there is still a very considerable portion of the high incidence of crime in the central segment of the city that must be explained on grounds other than these circumstances.'

25. R.E.L. Faris, *Social Disorganization*, New York 1955², pp. 337–339.

26. H.W. Dunham, *Community and Schizophrenia; an Epidemiological Analysis*, Detroit 1965, p. 222.

27. W.B. Miller, 'Lower Class Culture as a Generating Milieu of Gang Delinquency', *Journal of Social Issues*, 1958, 14, pp. 5–19.

28. ... 'the typical delinquent operates in the company of his peers, and delinquency thrives on group support. It has been estimated that between 60 and 90 per cent of all delinquent acts are committed with companions.' from: *The Challenge of Crime in a Free Society*, A Report by the President's Commission on Law Enforcement and Administration of Justice, Washington 1967, p. 66.
More than 81.8 percent of those brought into juvenile court had one or more companions; C.R. Shaw and H.D. Mc Kay, 'Social Factors in Juvenile Delin-

quency', in: *Report on the Causes of Crime by the National Commission on Law Observance and Enforcement*, Washington D.C. 1931, pp. 195–196.
Not only is gang delinquency (not necessarily of the highly organized type) a typical American phenomenon through the decades, it is manifest all over the world, in all types of political and social communities, See for example T.R. Fyvel, *Troublemakers: Rebellious Youth in an Affluent Society*, New York 1962. (Fyvel examined gang delinquency in several countries).

29. D. Bordua, 'A Critique of Sociological Interpretations of Gang Delinquency', in: M.E. Wolfgang, et al., (eds.), *The Sociology of Crime and Delinquency*, New York 1962, pp. 289–301. (Deleted from the second edition, 1970).

30. Amongst the discriminated minorities women (for whom all the indicators of discrimination hold except endogamy, if one agrees with the indicators put forward in M.B. Clinard, *Sociology of Deviant Behavior*, New York 1968[3], pp. 600–695) are persistently forgotten. In so far as women are members of ethnic or other minorities, they are discriminated against twice. In so far as their discrimination (historically, geographically and numerically by far the greatest and in many countries, like the Arabic ones, by far the severest) is omitted from serious and extensive scientific discussions, it may be considered the limit in discrimination.
For a more detailed discussion see C.I. Dessaur, 'Criminology's Blind Spot, or: a Dogged Bias in Criminological Research, article to be published in 1972.

31. Adaptation from D.R. Taft, 'Influence of the General Culture on Crime', *Federal Probation*, 30, 1966, pp. 19–22.

32. G.M. Sykes and D. Matza, Techniques of Neutralization: a Theory of Delinquency', *Am. Sociol. Review*, 22, 1957, pp. 664–670.
And: D. Matza, *Delinquency and Drift*, New York 1964.

33. L.T. Wilkins, *Evaluation of Penal Measures*, New York 1969, pp. 157–162 (for general conclusions based on this standpoint).

34. G.H. Mead, *Mind, Self and Society*, Chicago 1934.

35. Shakespeare, *As You Like It*, Act II, Sc. vii (*Jaques*).

36. For a book by a philosophical anthropologist in the same school of thought see G. Ryle, *The Concept of Mind*, London 1949.

37. M. Mead, *Sex and Temperament in Three Primitive Societies*, New York 1935. (To go into the criticism evoked by her theses would lead us too far here).

38. T. Parsons, 'Role Conflict and the Genesis of Deviance', in: B.J. Biddle and E.J. Thomas, (eds.), *Role Theory: Concepts and Research*, New York 1966, pp. 275–276.
For a detailed exposition of the conceptual framework of role-theory see the same book, pp. 23–63 (by the editors).

39. A.K. Cohen, 'The Sociology of the Deviant Act: Anomie Theory and Beyond', *American Sociol. Review*, 30, 1965.

40. A.K. Cohen, citing a Ph. D. dissertation by G. Grosser, in *Deviance and Control*, Englewood Cliffs, N.J. 1966, pp. 99–100.

41. M.B. Clinard, *Sociology of Deviant Behavior*, New York 1968[3], p. 70.

42. F. Tannenbaum, *Crime and the Community*, Boston 1938.

43. E.M. Lemert, *Human Deviance, Social Problems and Social Control*, Englewood Cliffs, N.J. 1967.

44. H. Garfinkel, 'Conditions of Successful Degradation Ceremonies', *American Journal of Sociol.*, 61, 1956, pp. 420–424.

45. D.H. Chapman, *Sociology and the Stereotype of the Criminal*, London 1968, pp. 197–238.

46. E. Goffman, *Stigma*, Englewood Cliffs, N.J. 1963.

47. E.M. Schur, *Crimes Without Victims: Deviant Behavior and Public Policy (Abortion; Homosexuality; Drug Addiction)*, Englewood Cliffs, N.J. 1965.)

48. A.K. Cohen, *op. cit.*, p. 111. A more detailed explication is given on pp. 103–104 in four points that briefly run as follows:

1. 'Human action does not typically happen all at once.'
2. 'The circumstances that determine movement along a particular path include both properties of the person and properties of the situation.'
3. 'Some of the circumstances that help to determine the development of the course of action are quite independent of events at earlier stages; some are outgrowths, often unanticipated, of events in earlier stages.'
4. The situational component in the interaction process consists largely of feedback from other actors.

49. A.K. Cohen, *op cit.*, p.114.

50. T. Shibutani, *Society and Personality: An Interaction Approach to Social Psychology*, Englewood Cliffs, N.J. 1961, e.g. pp. 118–119.

51. J.F. Short, Jr. and F.L. Strodtbeck, *Group Process and Gang Delinquency*, Chicago 1965.

52. A.K. Cohen, *op. cit.*, p. 66.

53. R.A. Cloward and Ll.B. Ohlin, *Delinquency and Opportunity: a Theory of Delinquent Gangs*, Glencoe, Ill. 1960.

54. D. Glaser, 'Criminality Theories and Behavioral Images', *American Journal of Sociol.*, 61, 1956, pp. 433–445.

55. D. Glaser, *op. cit.*, discusses diverse what he calls 'monistic theories' in criminology from that of the 'classical' (rationalistic; free will) school to economic determinism, biological and psychodynamic 'possession imagery', to extreme cultural determinism. He rejects them all, in favor of differential identification, 'The choice of another, from whose perspective we view our own behavior, is the process of identification.' This is 'imagery' taken from role-theory but the extreme cultural determinism that often goes with that theory is rejected by him. in favor of an eclectic psychological – sociological point of view.

56. L. Radzinowicz, at the Second United Nations Congress on the Prevention of Crime and Treatment of Offenders, London, August 1960. (Quoted by Reckless).

57. W.C. Reckless, *Criminal Behavior*, New York 1940, p. 255.

58. B. Wootton, *Social Science and Social Pathology*, London 1960[2] (1959), pp. 81–135. Her conclusion about the empirical validity of the twelve criminological hypotheses: 'All in all, therefore, this collection of studies, although chosen for its comparative methodological merit, produces only the most meagre, and dubiously supported, generalizations.'

59. T. Hirschi and H. C. Selvin, *Delinquency Research: an Appraisal of Analytic Methods*, New York 1967, pp. 23–27 conclude that Wootton grants the consistency of some studies, but questions their validity. (The assumption that they measure what they pretend to measure). This, however, is untenable with regard to several of these studies. The fact that a variable X under some conditions does affect delinquency and under other circumstances does not (or to a different degree) does not imply that no sound quantitative analysis has been done which may help theoretical progress in criminology.

60. W.C. Reckless, 'A Non-Causal Explanation: Containment Theory', *Excerpta Criminologica*, 1962, pp. 131–134.

61. H. Hyman, *Survey Design and Analysis*, Glencoe, Ill. 1955, Chapters 5–7.

62. T. Hirschi and H. C. Selvin, *op. cit.*, p. 38.

63. The 'modern' but by now already traditional criterion of falsifiability as a necessary constituent of scientific statements has been formulated by K. R. Popper. See his *The Logic of Scientific Discovery*, New York 1959 and *Conjectures and Refutations*, New York 1962.
A more moderate (and more recent) opinion on this subject is to be found in Th. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago 1970[2] (1962).

64. L. T. Wilkins, *Social Deviance: Social Policy, Action and Research*, London 1964. pp. 36–37. 'The theory of "multiple causation" is, then, no theory. At best it could be considered an anti-theory which proposes that no theory can be formed regarding crime.' (p. 37).

65. B. Lander, *Towards an Understanding of Juvenile Delinquency*, New York 1954, p. 71.

66. Hirschi and Selvin, *op. cit.*, pp. 131–133.

67. For an enumeratieon of these studies (some of them favorable, others dubious or unfavorable) see R. L. Burgess and R. L. Akers, 'A Differential Association-Reinforcement Theory of Criminal Behavior', *Social Problems*, 14, 1966, pp. 128–147, notes 4 and 5.
Their own theory is an eclectic one, combining 'modern behavior theory' with Sutherland's theory.

68. J.I. Kitsuse and D.C. Dietrick, 'Delinquent Boys: a Critique', *American Sociological Review*, 24, 1959, pp. 208–215.
G. Sykes and D. Matza, 'Techniques of Neutralization: a Theory of Delinquency', *American Sociological Review*, 22, 1957, pp. 664-670.
J.F. Short and F.L. Strodtbeck, *op. cit.*
W.B. Miller, *op. cit.*

69. A kind of labelling/stigmatizing/scapegoating theory with regard to mental illness that is isomorphic to those with regard to delinquency is to be found in T.S. Szasz, *Ideology and Insanity: Essays on the Psychiatric Dehumanization of Man*, New York 1970. See also his *The Myth of Mental Illness*, New York 1961.

CHAPTER VI

Conclusions

1. From the foregoing chapters, especially the last one, it may have become evident that there is still a long way to go before criminology will possess a core of theoretical statements that is acceptable to most researchers in the field. Because of the variety of research findings from sociology of penal law, sociology of deviant behavior, and in some cases (e.g. compulsive crimes) from human biology, that will have to be integrated, criminologists would be well advised, it seems to me, to use systems theory as an important, integrative conceptual framework.

Modern systems theory is of such a high level of generality or abstraction that findings from rather diverse branches of scientific endeavour can be interpreted with it. One of its founding fathers, K. Boulding,[1] distinguishes five levels at which systems (consisting of elements, concepts or variables) may function:

1. Static closed system (e.g. sticks and stones: no factors from outside the system can influence factors inside it, within a certain period of time);

2. Dynamic closed systems (like clockworks, our planetary system and such. There is only internal change within certain time limits);

3. Static open systems. These contain an 'input' and an 'output': elements enter and leave the system without, however, changing the system itself within time-limits. (E.g. a computer or a catalyser);

4. Dynamic open systems. These are changed because of their input and output. (E.g. human beings and human groups).

By considering our subject matter (legal organizations, societal actions, deviant subcultures and such) as dynamic open systems we have a tool for integrating many findings from criminology that are at present rather disjunct.

One of the main contributions of systems theory to modern sociology is in the subtle causal analysis it has for the complex type of open dynamic systems we are dealing with. For systems that are complexily adaptive and non-equilibrial (developing or vanishing) the first of the prerequisites for a causal link between variables (their association) as well as the second (the causal order)[2] may be of a hard to analyze type.

As for the association: this need not be a simple function of one or more variables, but it might be either of the kind that is described by step-functions (a nonlinear relation whereby a variable has no appreciable effect on others until its value has increased or decreased by some increment),[3] or it may be of the buffer mechanism type (in which the effects of a variable are delayed until some later point in the process), or it may be of the multiple factor type in which the independent variables should be *weighted* (according to their relative impact) or it may be of the circular causal chain (pseudo-feedback) type that is typical for self-regulating, goal-seeking systems. (In this case the effect of a variable returns indirectly to influence the original variable itself by way of one or more intervening variables.)

2. A first attempt at formulating a general theory of deviance, in particular of deviant behavior defined as criminal, in these systems theoretical terms has been made by L. T. Wilkins.[4] He stresses the point 'that the definition of what is deviance is considerably modified by the content of available information about certain behavior, the amount and channel of information'.[5]

This model, although still rudimentary, opens some perspectives for an integration of sociology of penal law and sociology of deviant behavior in criminology. It also gives some directions about what way we should look for (future) quantification of it: the kind of deviation-amplifying systems that are used in econometrics.[6]

Wilkins' model includes:

– An account for penal law selection and enforcement on the basis of information (as available or interpreted). The definition of certain acts (or people) as deviant, their being cut off from the general social system and culture by the very process of definition.

– The impact of the social action taken on the self-perception of the individual defined as deviant.

– The (further) development of subcultural (conflicting) norms in groups defined as deviant.

– The stimulus the latter may form for more forceful action by the conforming groups against the nonconformists.

– The establishing thereby of a self-perpetuating system (which may go round and round in an amplifying circuit).

Wilkins' model leaves out of its reach several important things:

a) the fact that the group who makes the definitions and enforces the sanctions of penal law consists almost exclusively of establishment males. It is in fact a semi-caste with a very unequal share of power in society (power defined as the possibility of taking decisions or influencing the decisions of others with regard to one's own group), as compared to lower class males (the main 'victims' of penal law definitions) and females of every class;

b) a specifiction of how and why the amplification process will reach its limits and deteriorate;

c) an indication of how to transform his qualitative model into a quantitative one.

Yet, in essence it is the same kind of approach that has been attempted in Chapter IV above and seems to me to be one of the most promising models from a programmatic point of view. (A similar model is to be found already in a Dutch dissertation: W. H. Nagel, *De criminaliteit van Oss*, The Hague 1949. Here the 'level of accepted behavior' was taken as the main independent variable.)

3. A meta-methodological problem that hampers theory-formation in criminology is the kind of training criminologists get. At worst they have not been trained as students in empirical science at all. At best they have studied some behavioral science and learned some statistical techniques and research design, but without a sufficient training in mathematics (in some of its newer branches that are adequate for behavioral systems) and in biological facts from genetics, physiology, endocrinology, that may be of extreme importance with regard to certain (compulsive) types of behavior defined as criminal.

Although this 'problem' is not one intrinsic to criminology, it may be one of great practical importance, since people are not motivated to do what they cannot do well. The mainly contemplative and programmatic contents of European criminological congresses are a testimony for this fact. (This is not a reproach to European criminologists, but to the academic training of behavioral scientists in general, in most European universities and to the training of criminologists in particular.)

4. Although I have tried to stress the possibility of criminology as a fundamental science, this should not be interpreted as if I had wanted to say that it is an independent science, with a specific subject matter and specific methods of discovery. Criminology cannot reach further than the main science of which it is a branch (or a combination of branches) can reach. Recent developments in sociology, which tend to create less substantial, more analytical subdivisions of this science, may be of great profit for criminology too. Instead of the sociology of this and the sociology of that (say: religion, the family, organizations, stratification, etcetera) we will probably have in the near future a sociology of social structure and social process, that is either concerned with dimensions of the social order (like differentiation and integration and context),[7] or with maintenance of the social order (to which belong socialization and social control), or with tensions in the social order (conflict and deviance), including extremism and war), or with changes in the social order (societal and organizational transformation).[8]

Sociology that is thus structured and oriented in its research will be much more fruitful for future criminological theory-formation than the traditional, substantially departmentalized sociology.

If this analytical approach is thoroughly adopted, it may also mean the end of criminology as we know it now: the science of (mainly) lower class males violating the penal norms of top males. It may become a science that includes in addition to the already traditionally defined white-collar crime (its first extension), a science that includes the phenomena of war, of environmental pollution and of discrimination, to name a few topics that have usually fallen outside its scope, but that may prove in the end to be of much greater relevance (both from a practical and a theoretical point of view) than the usually studied small-time criminal, or the sensational individual.

NOTES

1. K. Boulding, 'General Systems Theory; the Skeleton of Science', *Management Science*, 2, 1956, pp. 197–208. See also: L. von Bertalanffy, 'General System Theory: A Critical Review', Yearbook of the Society for General Systems Research, 7, 1962, pp. 1–20.

2. See Chapter V above, paragraph 15.

3. W. Buckley, *Sociology and Modern Systems Theory*, Englewood Cliffs, N.J. 1967, pp. 66–80.

4. L. T. Wilkins, *Social Deviance: Social Policy, Action and Research*, London 1964, pp. 45–104.
5. Wilkins, *op. cit.*, p. 91.
6. Wilkins, *op. cit.*, pp. 92–93.
7. Under the heading of 'context' fall structural effects of the social order, such as the distribution of delinquency in the social class structure.
8. This approach to sociology can be found in P. Orleans (ed.), *Social Structure and Social Process: an Introductory Reader*, Boston, Mass. 1970. See pp. v–vii.

Summary

In order to arrive at theory-formation in the strict methodological sense, criminologists ought to maintain a sharp distinction between criminology as a fundamental science and criminology as an applied science (the latter being bound to current ideologies and ethics, acute social problems and individual needs).

As a fundamental science criminology is mainly a combination of two branches of sociology: sociology of penal law and sociology of deviant behavior. It is hampered in its growth by several factors:

a) the inadequate training of most (European) criminologists;

b) the lack of mathematical and biological training of those criminologists who are behavioral scientists at least;

c) the misuse of (inferential) statistics as if it were a sufficient theory in itself, instead of a probabilistic mathematical system, apt for testing hypotheses but requiring interpretation of its results via other theoretical models;

d) the use of typologies that do not make clear the four basic categories of deviant behavior defined as criminal; viz. behavior for which there have or have not been defined objective indicators, and behavior that may be considered as being at the discretion of the actor or not;

e) the continued use of explanative criminological models or 'theories' that have been falsified or rejected convincingly, like most variants of the (Freudian) psychodynamic model (taking psychological variables as the main independent ones);

f) rejection of biological explanations for at least certain types of behavior defined as criminal on irrelevant grounds (stigma of the fascists' interest in this kind of criminology);

g) the muddle of causal models and especially the repleteness of false criteria of non-causality in criminological research;

h) the use of prediction in criminology as if it were a 'school of thought' instead of a statistical technique or an aspect of every therory;

i) the difficulties of translating the most promising sociological model or 'theory'; that of cultural transmission of deviant behavior, into a formalized language.

With the integration of findings from sociology of penal law, sociology of deviant behavior and maybe some basic biological data in view, criminologists would probably do best to use modern systems theory as their main tool. This theory is of a sufficient level of generality to be able to incorporate elements from different theories of a lower level of abstraction.

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